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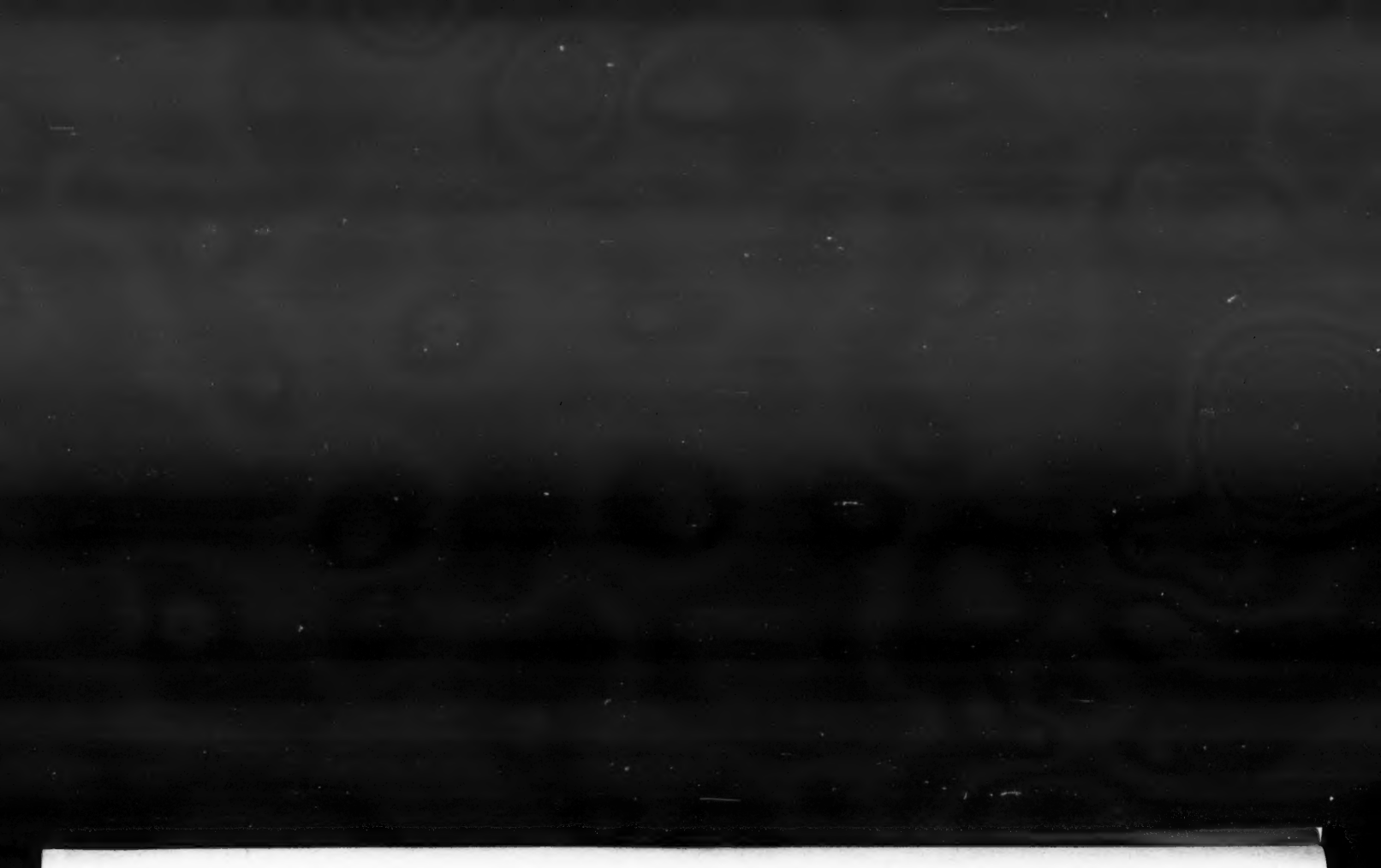

THE

AMERICAN JOURNAL OF SCIENCE

[FOURTH SERIES.]

HUBERT ANSON NEWTON.

[Read before the National Academy of Sciences, in April, 1897.—The portrait accompanying this notice is a reproduction of a photograph taken by a member of the family in the spring of 1894.]

HUBERT ANSON NEWTON was born on March 19th, 1830, at Sherburne, N. Y., and died at New Haven, Conn., on the 12th day of August, 1896. He was the fifth son of a family of seven sons and four daughters, children of William and Lois (Butler) Newton. The parents traced their ancestry back to the first settlers of Massachusetts and Connecticut,* and had migrated from the latter to Sherburne, when many parts of central New York were still a wilderness. They both belonged to families remarkable for longevity, and lived themselves to the ages of ninety-three and ninety-four years. Of the children, all the sons and two daughters were living as recently as the year 1889, the youngest being then fifty-three years of age. William Newton was a man of considerable enterprise, and undertook the construction of the Buffalo section of the Erie canal, as well as other work in canal and railroad construction in New York and Pennsylvania. In these constructions he is said to have relied on his native abilities to think out for himself the solution of problems which are generally a matter of technical training. His wife was remarkable for great strength of character united with a quiet temperament and well-balanced mind, and was noted among her neighbors for her mathematical powers.

* Richard Butler, the great-grandfather of Lois Butler, came over from England before 1633, and was one of those who removed from Cambridge to Hartford. An ancestor of William Newton came directly from England to the New Haven colony about the middle of the same century.

Young Newton, whose mental endowments were thus evidently inherited, and whose controlling tastes were manifested at a very early age, fitted for college at the schools of Sherburne, and at the age of sixteen entered Yale College in the class graduating in 1850. After graduation he pursued his mathematical studies at New Haven and at home, and became tutor at Yale in January, 1853, when on account of the sickness and death of Professor Stanley the whole charge of the mathematical department devolved on him from the first.

In 1855, he was appointed professor of mathematics at the early age of twenty-five. This appointment testifies to the confidence which was felt in his abilities, and is almost the only instance in which the Yale Corporation has conferred the dignity of a full professorship on so young a man.

This appointment being accompanied with a leave of absence for a year, in order to give him the opportunity to study in Europe, it was but natural that he should be attracted to Paris, where Chasles was expounding at the Sorbonne that modern higher geometry of which he was to so large an extent the creator, and which appeals so strongly to the sense of the beautiful. And it was inevitable that the student should be profoundly impressed by the genius of his teacher, and by the fruitfulness and elegance of the methods which he was introducing. The effect of this year's study under the inspiring influence of such a master is seen in several contributions to the Mathematical Monthly during its brief existence in the years 1858–61. One of these was a problem which attracted at once the attention of Cayley, who sent a solution. Another was a discussion of the problem "to draw a circle tangent to three given circles," remarkable for his use of the principle of inversion. A third was a very elaborate memoir on the construction of curves by the straight edge and compasses, and by the straight edge alone. These early essays in geometry show a mind thoroughly imbued with the spirit of modern geometry, skilful in the use of its methods, and eager to extend the bounds of our knowledge.

Nevertheless, although for many years the higher geometry was with him a favorite subject of instruction for his more advanced students, either his own preferences, or perhaps rather the influence of his environment, was destined to lead him into a very different field of research. In the attention which has been paid to astronomy in this country we may recognize the history of the world repeating itself in a new country in respect to the order of the development of the sciences, or it may be enough to say that the questions which nature forces on us are likely to get more attention in a new country and a bustling age, than those which a reflective mind

puts to itself, and that the love of abstract truth which prompts to the construction of a system of doctrine, and the refined taste which is a critic of methods of demonstration, are matters of slow growth. At all events, when Professor Newton was entering upon his professorship, the study of the higher geometry was less consonant with the spirit of the age in this country than the pursuit of astronomical knowledge, and the latter sphere of activity soon engrossed his best efforts.

Yet it was not in any of the beaten paths of astronomers that Professor Newton was to move. It was rather in the wilds of a *terra incognita*, which astronomers had hardly troubled themselves to claim as belonging to their domain, that he first labored to establish law and order. It was doubtless not by chance that he turned his attention to the subject of shooting stars. The interest awakened in this country by the stupendous spectacle of 1833, which was not seen in Europe, had not died out, as is abundantly shown by inspection of the indexes of this Journal. This was especially true at New Haven, where Mr. Edward C. Herrick was distinguished for his indefatigable industry both in personal observation and in the search for records of former showers. A rich accumulation of material was thus awaiting development. In 1861, the Connecticut Academy of Arts and Sciences appointed a committee "to communicate with observers in various localities for combined and systematic observations upon the August and November meteors." In this committee Professor Newton was preëminently active. He entered zealously upon the work of collecting material by personal observation and correspondence and by organizing corps of observers of students and others, and at the same time set himself to utilize the material thus obtained by the most careful study. The value of the observations collected was greatly increased by a map of the heavens for plotting meteor-paths, which was prepared by Professor Newton and printed at the expense of the Connecticut Academy for distribution among observers.

By these organized efforts, in a great number of cases, observations were obtained on the same meteor as seen from different places, and the actual path in the atmosphere was computed by Professor Newton. In a paper published in 1865* the vertical height of the beginning and the end of the visible part of the path is given for more than one hundred meteors observed on the nights of August 10th and November 13th, 1863. It was shown that the average height of the November meteors is fifteen or twenty miles higher than that of the August meteors, the former beginning in the mean at a height of ninety-six miles and ending at sixty-one, the latter beginning at seventy and ending at fifty-six.

* This Journal, II, vol xl, p. 250.

We mention this paper first, because it seems to represent the culmination of a line of activity into which Professor Newton had entered much earlier. We must go back to consider other papers which he had published in the mean time.

His first papers on this subject, 1860–62,* were principally devoted to the determination of the paths and velocities of certain brilliant meteors or fireballs, which had attracted the attention of observers in different localities. Three of these appeared to have velocities much greater than is possible for permanent members of the solar system. To another a particular interest attached as belonging to the August shower, although exceptional in size. For this he calculated the elements of the orbit which would give the observed path and velocity. But the determination of the velocity in such cases, which depends upon the estimation by the observers of the time of flight, is necessarily very uncertain, and at best affords only a lower limit for the value of the original velocity of the body before it encountered the resistance of the earth's atmosphere. This would seem to constitute an insuperable difficulty in the determination of the orbits of meteoroids, to use the term which Professor Newton applied to these bodies, before they enter the earth's atmosphere to appear for a moment as luminous meteors. Yet it has been completely overcome in the case of the November meteors or Leonids, as they are called from the constellation from which they appear to radiate. This achievement constitutes one of the most interesting chapters in the history of meteoric science, and gives the subject an honorable place among the exact sciences.

In the first place, by a careful study of the records, Professor Newton showed that the connection of early showers with those of 1799 and 1833 had been masked by a progressive change in the time of the year in which the shower occurs. This change had amounted to a full month between A. D. 902, when the shower occurred on October 13, and 1833, when it occurred on November 13. It is in part due to the precession of the equinoxes, and in part to the motion of the node where the earth's orbit meets that of the meteoroids. This motion must be attributed to the perturbations of the orbits of the meteoroids which are produced by the attractions of the planets, and being in the direction opposite to that of the equinoxes, Professor Newton inferred that the motion of the meteoroids must be retrograde.

The showers do not, however, occur whenever the earth passes the node, but only when the passage occurs within a year or two before or after the termination of a cycle of 33.25 years. This number is obtained by dividing the interval between the

* This Journal, II, xxx, p. 186; xxxii, p. 448; and xxxiii, p. 338.

showers of 902 and 1833 by 28, the number of cycles between these dates, and must therefore be a very close approximation. For if these showers did not mark the precise end of cycles, the resultant error would be divided by 28. Professor Newton showed that this value of the cycle requires that the number of revolutions performed by the meteoroids in one year should be either $2 \pm \frac{1}{33 \cdot 25}$ or $1 \pm \frac{1}{33 \cdot 25}$ or $\frac{1}{33 \cdot 25}$. In other words, the periodic time of the meteoroids must be either 180.0 or 185.4 or 354.6 or 376.6 days, or 33.25 years. Now the velocity of any body in the solar system has a simple relation to its periodic time and its distance from the sun. Assuming, therefore, any one of these five values of the periodic time, we have the velocities of the Leonids at the node very sharply determined. From this velocity, with the position of the apparent radiant, which gives the direction of the relative motion, and with the knowledge that the heliocentric motion is retrograde, we may easily determine the orbit.

We have, therefore, five orbits from which to choose. The calculation of the secular motion of the node due to the disturbing action of the planets, would enable us to decide between these orbits.

Such are the most important conclusions which Professor Newton derived from the study of these remarkable showers, interesting not only from the magnificence of the spectacle occasionally exhibited, but in a much higher degree from the peculiarity in the periodic character of their occurrence, which affords the means of the determination of the orbit of the meteoroids with a precision which would at first sight appear impossible.

Professor Newton anticipated a notable return of the shower in 1866, with some precursors in the years immediately preceding, a prediction which was amply verified. In the mean time he turned his attention to the properties which belong to shooting stars in general, and especially to those average values which relate to large numbers of these bodies not belonging to any particular swarm.

This kind of investigation Maxwell has called *statistical*, and has in more than one passage signalized its difficulties. The writer recollects a passage of Maxwell which was pointed out to him by Professor Newton, in which the author says that serious errors have been made in such inquiries by men whose competency in other branches of mathematics was unquestioned. Doubtless Professor Newton was very conscious of the necessity of caution in these inquiries, as is indeed abundantly evident from the manner in which he expressed his conclusions; but the writer is not aware of any passage in which he has afforded an illustration of Maxwell's remark.

The results of these investigations appeared in an elaborate memoir "On Shooting Stars," which was read to the National Academy in 1864, and appeared two years later in the Memoirs of the Academy.* An abstract was given in this Journal in 1865.† The following are some of the subjects treated, with some of the more interesting results:

The distribution of the apparent paths of shooting stars in azimuth and altitude.

The vertical distribution of the luminous part of the real paths. The value found for the mean height of the middle point of the luminous path was a trifle less than sixty miles.

The mean length of apparent paths.

The mean distance of paths from the observer.

The mean foreshortening of paths.

The mean length of the visible part of the real paths.

The mean time of flight as estimated by observers.

The distribution of the orbits of meteoroids in the solar system.

The daily number of shooting stars, and the density of the meteoroides in the space which the earth traverses. The average number of shooting stars which enter the atmosphere daily, and which are large enough to be visible to the naked eye, if the sun, moon and clouds would permit it, is more than seven and a half millions. Certain observations with instruments seem to indicate that this number should be increased to more than four hundred millions, to include telescopic shooting stars, and there is no reason to doubt that an increase of optical power beyond that employed in these observations would reveal still larger numbers of these small bodies. In each volume of the size of the earth, of the space which the earth is traversing in its orbit about the sun, there are as many as thirteen thousand small bodies, each of which is such as would furnish a shooting star visible under favorable circumstances to the naked eye.

These conclusions are certainly of a startling character, but not of greater interest than those relating to the velocity of meteoroids. There are two velocities to be considered, which are evidently connected, the velocity relative to the earth, and the velocity of the meteoroids in the solar system. To the latter, great interest attaches from the fact that it determines the nature of the orbit of the meteoroid. A velocity equal to that of the earth, indicates an orbit like that of the earth; a velocity $\sqrt{2}$ times as great, a parabolic orbit like that of most comets, while a velocity greater than this indicates a hyperbolic orbit.

Professor Newton sought to form an estimate of this critical quantity in more than one way. That on which he placed

* Vol. i, 3d memoir. † II, xxxix, 193.

most reliance was based on a comparison of the numbers of shooting stars seen in the different hours of the night. It is evident that in the morning, when we are in front of the earth in its motion about the sun, we should see more shooting stars than in the evening, when we are behind the earth; but the greater the velocity of the meteoroids compared with that of the earth, the less the difference would be in the numbers of evening and morning stars.*

After a careful discussion of the evidence Professor Newton reached the conclusion that "we must regard as almost certain (on the hypothesis of an equable distribution of the directions of absolute motions), that the mean velocity of the meteoroids exceeds considerably that of the earth; that the orbits are not approximately circular, but resemble more the orbits of comets."

This last sentence, which is taken from the abstract published in this Journal in 1865, and is a little more definitely and positively expressed than the corresponding passage in the original memoir, indicating apparently that the author's conviction had been growing more positive in the interval, or at least that the importance of the conclusion had been growing upon him, embodies what is perhaps the most important result of the memoir, and derives a curious significance from the discoveries which were to astonish astronomers in the immediate future.

The return of the November or Leonid shower in 1865, and especially in 1866, when the display was very brilliant in Europe, gave an immense stimulus to meteoric study, and an especial prominence to this group of meteoroids. "Not since the year 1759," says Schiaparelli, "when the predicted return of a comet first took place, had the verified prediction of a periodic phenomenon made a greater impression than the magnificent spectacle of November, 1866. The study of cosmic meteors thereby gained the dignity of a science, and took finally an honorable place among the other branches of astronomy."† Professor J. C. Adams, of Cambridge, England, then took up the calculation of the perturbations determining the motion of the node. We have seen that Professor Newton had shown that the periodic time was limited to five sharply determined values, each of which with the other data would

* It may not be out of place to notice here an erratum which occurs both in the Memoirs of the National Academy and in the abstract in this Journal, and which the writer finds marked in a private copy of Professor Newton's. In the table on page 20 of the memoir and page 206 of the abstract, the column of numbers under the head "hour of the night" should be inverted. There is another displacement in the table in the memoir, which is, however, corrected in the abstract.

† Schiaparelli, Entwurf einer astronomishen Theorie der Sternschnuppen, p. 55.

give an orbit, and that the true orbit could be distinguished from the other four by the calculation of the secular motion of the node.

Professor Adams first calculated the motion of the node due to the attractions of Jupiter, Venus, and the Earth for the orbit having a period of 354·6 days. This amounted to a little less than 12′ in 33·25 years. As Professor Newton had shown that the dates of the showers require a motion of 29′ in 33·25 years, the period of 354·6 days must be rejected. The case would be nearly the same with a period of 376·6 days, while a period of 180 or 185·4 days would give a still smaller motion of the node. Hence, of the five possible periods indicated by Professor Newton, four were shown to be entirely incompatible with the motion of the node, and it only remained to examine whether the fifth period, viz: that of 33·25 years, would give a motion of the node in accordance with the observed value. As this period gives a very long ellipse for the orbit, extending a little beyond the orbit of Uranus, it was necessary to take account of the perturbations due to that planet and to Saturn. Professor Adams found 28′ for the motion of the node. As this value must be regarded as sensibly identical with Professor Newton's 29′ of observed motion, no doubt was left in regard to the period of revolution or the orbit of the meteoroids.*

About this time, M. Schiaparelli was led by a course of reasoning similar to Professor Newton's to the same conclusion,—that the mean velocity of the meteoroids is not very different from that due to parabolic orbits. In the course of his speculations in regard to the manner in which such bodies might enter the solar system, the questions suggested themselves: whether meteoroids and comets may not have a similar origin; whether, in case a swarm of meteoroids should include a body of sufficient size, this would not appear as a comet; and whether some of the known comets may not belong to streams of meteoroids. Calculating the orbit of the Perseids, or August meteoroids, from the radiant point, with the assumption of a nearly parabolic velocity, he found an orbit very similar to that of the great comet of 1862, which may therefore be considered as one of the Perseids,—probably the largest of them all.†

At that time no known cometic orbit agreed with that of the Leonids, but a few months later, as soon as the definitive elements of the orbit of the first comet of 1866 were published, their resemblance to those of the Leonids, as calculated for the period of 33·25 years, which had been proved to be the correct

* Monthly Notices Roy. Ast. Soc., vol. xxvii, p. 247.
† Entwurf, etc., pp. 49–54.

value, was strikingly manifested, attracting at once the notice of several astronomers.

Other relations of the same kind have been discovered later, of which that of Biela's comet and the Andromeds is the most interesting, as we have seen the comet breaking up under the influence of the sun; but in no case is the coincidence so striking as in that of the Leonids, since in no other case is the orbit of the meteoroids completely known, independently of that of the comet, and without any arbitrary assumption in regard to their periodic time.

The first comet of 1866 is probably not the only one belonging to the Leonid stream of meteoroids. Professor Newton has remarked that the Chinese annals mention two comets which passed rapidly in succession across the sky in 1366, a few days after the passage of the earth through the node of the Leonid stream, which was marked in Europe by one of the most remarkable star-showers on record. The course of these comets, as described by the annalists, was in the line of the Leonid stream.*

This identification of comets with meteors or shooting-stars marks an epoch in the study of the latter. Henceforth, they must be studied in connection with comets. It was presumably this discovery which led Professor Newton to those statistical investigations respecting comets, which we shall presently consider. At this point, however, at the close as it were of the first chapter in the history of meteoric science, it seems not unfitting to quote the words of an eminent foreign astronomer, written about this time, in regard to Professor Newton's contributions to this subject. In an elaborate memoir in the *Comptes Rendus*, M. Faye says, with reference to our knowledge of shooting-stars and their orbits, "we may find in the works of M. Newton, of the United States, the most advanced expression of the state of science on this subject, and even the germ, I think, of the very remarkable ideas brought forward in these last days by M. Schiaparelli and M. Le Verrier."†

The first fruit of Professor Newton's statistical studies on comets appeared in 1878 in a paper "On the Origin of Comets." In this paper he considers the distribution in the solar system of the known cometic orbits, and compares it with what we might expect on either of two hypotheses: that of Kant, that the comets were formed in the evolution of the solar system from the more distant portion of the solar nebula; and that of Laplace, that the comets have come from the stellar spaces and in their origin had no relation to the solar system.

* This Journal II, xliii, p. 298, and xlv, p. 91, or Encycl. Britann., article Meteor.

† Comptes Rendus, T. lxiv, p. 551.

In regard to the distribution of the aphelia, he shows that, except so far as modified by the perturbations due to the planets, the theory of internal origin would require all the aphelia to be in the vicinity of the ecliptic,—the theory of external origin would make all directions of the aphelia equally probable, *i. e.*, the distribution in latitude of the aphelia should be that in which the frequency is as the cosine of the latitude. The actual distribution comes very near to this, but as the effect of perturbations would tend to equalize the distribution of aphelia in all directions, Professor Newton does not regard this argument as entirely decisive. He remarks, however, that if Kant's hypothesis be true, the comets must have been revolving in their orbits a very long time, and the process of the disintegration of comets must be very slow.

In regard to the distribution of the orbits in inclination, the author shows that the theory of internal origin would make all inclinations equally probable,—the theory of external origin would make all directions of the normal to the plane of the orbit equally probable. On the first hypothesis, therefore, we should expect a uniform distribution in inclination; on the second, a frequency proportioned to the sine of the inclination. It was shown by a diagram in which the actual and the two theoretical distributions are represented graphically, that the actual distribution agrees pretty well with the theory of external origin and not at all with that of internal origin. It was also shown that the curve of actual distribution cannot be made to agree with Kant's hypothesis by any simple and reasonable allowances for perturbations. On the other hand, if we assume the external origin of comets, and ask how the curve of sines must be modified in order to take account of perturbations, it is shown that the principal effect will be to increase somewhat the number of inclinations between 90° and 135° at the expense of those between 45° and 90°. It is apparent at once from the diagram that such a change would make a very good agreement between the actual and theoretical curves, the only important difference remaining being due to comets of short periods, which mostly have small inclinations with direct motion. These should not weigh very much, the author observes, in the general question of the distribution of inclinations, because they return so frequently and are so easily detected that their number in a list of observed comets is out of all proportion to their number among existing comets. But this group of comets of short periods can easily be explained on the theory of an external origin. For such comets must have lost a large part of their velocity by the influence of a planet. This is only likely to happen when a comet overtakes the planet and passes in front of it. This implies that its orig-

inal motion was direct and in an orbit of small inclination to that of the planets, and although it may lose a large part of its velocity, its motion will generally remain direct and in a plane of small inclination. This very interesting case of the comets of short periods and small inclinations, which was treated rather briefly in this paper, was discussed more fully by Professor Newton at the meeting of the British Association in the following year.*

Many years later, Professor Newton returned to the same general subject in a very interesting memoir "On the Capture of Comets by Planets; especially their Capture by Jupiter," which was read before the National Academy in 1891, and appeared in the Memoirs of the Academy two years later.† It also appeared in this Journal in the year in which it was read.‡ This contains the results of careful statistical calculations on the effect of perturbations on orbits of comets originally parabolic. It corroborates the more general statements of the paper "On the Origin of Comets," giving them a precise quantitative form. One or two quotations will give some idea of the nature of this very elaborate and curious memoir, in which, however, the results are largely presented in the form of diagrams.

On a certain hypothesis regarding an original equable distribution of comets in parabolic orbits about the sun, it is shown that "if in a given period of time a thousand million comets come in parabolic orbits nearer to the sun than Jupiter, 126 of them will have their orbits changed" by the action of that planet "into ellipses with periodic times less than one-half that of Jupiter; 839 of them will have their orbits changed into ellipses with periodic times less than that of Jupiter; 1701 of them will have their orbits changed into ellipses with periodic times less than once and a half that of Jupiter, and 2670 of them will have their orbits changed into ellipses with periodic times less than twice that of Jupiter." A little later, Professor Newton considers the question, which he characterizes as perhaps more important, of the direct or retrograde motion of the comets after such perturbations. It is shown that of the 839 comets which have periodic times less than Jupiter, 203 will have retrograde motions, and 636 will have direct motions. Of the 203 with retrograde motion, and of the 636 with direct motion, 51 and 257, respectively, will have orbits inclined less than 30° to that of Jupiter.

We have seen that the earliest of Professor Newton's more important studies on meteors related to the Leonids, which at

* Rep't Brit. Assoc. Adv. Sci. for 1879, p. 272.
† Mem. Nat. Acad., vol. vi, 1st memoir.
‡ This Journal, III, xlii, pp. 183 and 482.

that time far surpassed all other meteoric streams in interest. One of his later studies related to another stream which in the mean time had acquired great importance. The identification of the orbit of the Andromed meteors with that of Biela's comet, which we have already mentioned, gave these bodies a unique interest, as the comet had been seen to break up under the influence of the sun. Here the evolution of meteoroids was taking place before our eyes; and this interest was heightened by the showers of 1872 and 1885, which in Europe seem to have been unsurpassed in brilliancy by any which have occurred in this century.

The phenomena of each of these showers were carefully discussed by Professor Newton. Among the principal results of his paper on the latter shower are the following:*

The time of the maximum frequency of meteors was Nov. 27, 1885, $6^h\ 15^m$ Gr. m. t. The estimated number per hour visible at one place was then 75,000. This gives a density of the meteoroids in space represented by one to a cube of twenty miles edge. Three hours later the frequency had fallen to one-tenth of the maximum value. The really dense portion of the stream through which we passed was less than 100,000 miles in thickness, and nearly all would be included in a thickness of 200,000 miles.

A formula is given to express the effect of the earth's attraction on the approaching meteoroids in altering the position of the radiant. This is technically known as the zenithal attraction, and is quite important in the case of these meteors on account of their small relative velocity. The significance of the formula may be roughly expressed by saying that the earth's attraction changes the radiant of the Biela meteors, toward the vertical of the observer, one-tenth of the observed zenith distance of the radiant, or more briefly, that the zenithal attraction for these meteors is one-tenth of the observed zenith distance. The radiant even after the correction for zenithal attraction, and another for the rotation of the earth on its axis, is not a point but an area of several degrees diameter. The same has been observed in regard to other showers, but the result comes out more distinctly in the present case because the meteors were so numerous and the shower so carefully observed.

This implies a want of parallelism in the paths of the meteors, and it is a very important question whether it exists before the meteoroids enter our atmosphere, or whether it is due to the action of the atmosphere.

Professor Newton shows that it is difficult to account for so large a difference in the original motions of the meteoroids, and

* This Journal, III, xxxi, p. 409.

thinks it reasonable to attribute a large part of the want of parallelism to the action of the atmosphere on bodies of an irregular form, such as we have every reason to believe that the meteoroids have, when they enter our atmosphere. The effect of the heat generated will be to round off the edges and prominent parts, and to reduce the meteor to a form more and more spherical. It is, therefore, quite natural that the greater portion of the curvature of the paths should be in the invisible portion and thus escape our notice. It is only in exceptional cases that the visible path is notably curved.

But the great interest of the paper centers in his discussion of the relation of this shower to preceding showers, and to the orbit of Biela's comet. The changes in the date of the shower (from Dec. 6 to Nov. 27) and in the position of the radiant are shown to be related to the great perturbations of Biela's comet in 1794, 1831, and 1841–2. The showers observed by Brandes, Dec. 6th, 1798, by Herrick, Dec. 7th, 1838, and by Heis, Dec. 8th and 10th, 1847, are related to the orbit of Biela's comet as it was in 1772; while the great showers of 1872 and 1885, as well as a trifling display in 1867, are related to the orbit of 1852.*

Assuming, then, that the meteoroids which we met on the 27th of November, 1872, did not leave the immediate neighborhood of the Biela comet before 1841·5, we seem to have the data for a very precise determination of their orbit between those dates. The same is true of those which we met in 1885. The computation of these orbits, the author remarks, may possibly give evidence for or against the existence of a resisting medium in the solar system.

In his last public utterance on the subject of meteors, which was on the occasion of the recent sesquicentennial celebration of the American Philosophical Society, Professor Newton returns to the Biela meteoroids, and finds in the scattering which they show in the plane of their orbit the proof of a disturbing force in that plane, and therefore not due to the planets. The force exerted by the sun appears to be modified somewhat as we see it in the comet's tails, where indeed the attraction is changed into a repulsion. Something of the same sort on a smaller scale relatively to the mass of the bodies appears to modify the sun's action on the meteoroids.

In 1888 Professor Newton read a paper before the National Academy "Upon the relation which the former Orbits of those Meteorites that are in our collections, and that were seen

*It is a curious coincidence that the original discoverer of the December shower as a periodic phenomenon, Mr. Edward C. Herrick, should have been (with a companion, Mr. Francis Bradley,) the first to observe that breaking up of the parent body which was destined to reinforce the meteoric stream in so remarkable a manner. See this Journal, III, xxxi, pp. 85 and 88.

to fall, had to the Earth's Orbit." This was based upon a very careful study of more than 116 cases for which we have statements indicating more or less definitely the direction of the path through the air, as well as 94 cases in which we only know the time of the fall. The results are expressed in the following three propositions:

1. The meteorites which we have in our cabinets and which were seen to fall were originally (as a class, and with a very small number of exceptions) moving about the sun in orbits that had inclinations less than 90°; that is, their motions were direct, not retrograde.

2. The reason why we have only this class of stones in our collections is not one wholly or even mainly dependent on the habits of men; nor on the times when men are out of doors; nor on the places where men live; nor on any other principle of selection acting at or after the arrival of the stones at the ground. Either the stones which are moving in the solar system across the earth's orbit move in general in direct orbits; or else for some reason the stones which move in retrograde orbits do not in general come through the air to the ground in solid form.

3. The perihelion distances of nearly all the orbits in which these stones moved were not less than 0·5 nor more than 1·0, the earth's radius vector being unity.

Professor Newton adds, that it seems a natural and proper corollary to these propositions (unless it shall appear that stones meeting the earth are destroyed in the air) that the larger meteorites moving in our solar system are allied much more closely with the group of comets of short period than with comets whose orbits are nearly parabolic. All the known comets of shorter periods than 33 years move about the sun in direct orbits that have moderate inclinations to the ecliptic. On the contrary, of the nearly parabolic orbits that are known only a small proportion of the whole number have small inclinations with direct motion.

We have briefly mentioned those papers which seem to constitute the most important contributions to the science of meteors and comets. To fully appreciate Professor Newton's activity in this field, it would be necessary to take account of his minor contributions. These are given in the annexed bibliography, where it will be seen that more than half of the entries relate to these subjects.

Most interesting and instructive to the general reader are his utterances on occasions when he has given a résumé of our knowledge on these subjects or some branch of them, as in the address "On the Meteorites, the Meteors, and the Shooting

Stars," which he delivered in 1886 as retiring president of the American Association for the Advancement of Science, or in certain lectures in the public courses of the Sheffield Scientific School of Yale University, entitled "The story of Biela's Comet" (1874), "The relation of Meteorites to Comets" (1876), "The Worship of Meteorites" (1889), or in the articles on Meteors in the Encyclopædia Britannica and Johnson's Cyclopædia.

If we ask what traits of mind and character are indicated by these papers, the answer is not difficult. Professor Klein has divided mathematical minds into three leading classes: the logicians, whose pleasure and power lies in subtility of definition and dialectic skill; the geometers, whose power lies in the use of the space-intuitions; and the formalists, who seek to find an algorithm for every operation.* Professor Newton evidently belonged to the second of these classes, and his natural tastes seem to have found an equal gratification in the development of a system of abstract geometric truths, or in the investigation of the concrete phenomena of nature as they exist in space and time.

But these papers show more than the type of mind of the author; they give no uncertain testimony concerning the character of the man. In all these papers we see a love of honest work, an aversion to shams, a distrust of rash generalizations and speculations based on uncertain premises. He was never anxious to add one more guess on doubtful matters in the hope of hitting the truth, or what might pass as such for a time, but was always willing to take infinite pains in the most careful test of every theory. To these qualities was joined a modesty which forbade the pushing of his own claims, and desired no reputation except the unsought tribute of competent judges. At the close of his article on meteors in the Encyclopædia Britannica, which has not the least reference to himself as a contributor to the science, he remarks that "meteoric science is a structure built stone by stone by many builders." We may add that no one has done more than himself to establish the foundations of the science, and that the stones which he has laid are not likely to need relaying.

The value of Professor Newton's work has been recognized by learned societies and institutions both at home and abroad. He received the honorary degree of Doctor of Laws from the University of Michigan in 1868. He was president of the section of Mathematics and Astronomy in the American Association for the Advancement in Science in 1875, and president of the Association in 1885. On the first occasion he delivered an address entitled "A plea for the study of pure mathematics";

* Lectures on Mathematics (Evanston), p. 2.

on the second the address on Meteorites, etc., which we have already mentioned. Of the American Mathematical Society he was vice-president at the time of his death. In 1888 the J. Lawrence Smith gold medal was awarded to him by the National Academy for his investigations on the orbits of meteoroids. We may quote a sentence or two from his reply to the address of presentation, so characteristic are they of the man that uttered them: "To discover some new truth in nature," he said, "even though it concerns the small things in the world, gives one of the purest pleasures in human experience. It gives joy to tell others of the treasure found."

Besides the various learned societies in our own country of which he was a member, including the American Academy of Arts and Sciences from 1862, the National Academy of Sciences from its foundation in 1863, the American Philosophical Society from 1867, he was elected in 1872 Associate of the Royal Astronomical Society of London, in 1886 Foreign Fellow of the Royal Society of Edinburgh, and in 1892 Foreign Member of the Royal Society of London.

But the studies which have won for their author an honorable reputation among men of science of all countries, form only one side of the life of the man whom we are considering. Another side, probably the most important, is that in which he was identified with the organic life of the College and University with which he had been connected from a very early age. In fact, we might almost call the studies which we have been considering, the recreations of a busy life of one whose serious occupation has been that of an instructor. If from all those who have come under his instruction we should seek to learn their personal recollections of Professor Newton, we should probably find that the most universal impression made on his students was his enthusiastic love of the subject which he was teaching.

A department of the University in which he took an especial interest was the Observatory. This was placed under his direction at its organization, and although he subsequently resigned the nominal directorship, the institution remained virtually under his charge, and may be said to owe its existence in large measure to his untiring efforts and personal sacrifice in its behalf.

One sphere of activity in the Observatory was suggested by a happy accident which Professor Newton has described in this Journal, September, 1893. An amateur astronomer in a neighboring town, Mr. John Lewis, accidentally obtained on a stellar photograph the track of a large meteor. He announced in the newspapers that he had secured such a photograph, and requested observations from those who had seen its flight. The

photographic plate, with letters received from various observers, were placed in Professor Newton's hands, and were discussed in the paper mentioned. The advantages of photographic observations were so conspicuous that Professor Newton was anxious that the Observatory should employ this method of securing the tracks of meteors. With the aid of an appropriation granted by the National Academy from the income of the J. Lawrence Smith fund, a battery of cameras was mounted on an equatorial axis. By this means, a number of meteor-tracks have been obtained of the August meteors, and in one case, through a simultaneous observation by Mr. Lewis in Ansonia, Professor Newton was able to calculate the course of the meteor in the atmosphere with a probable error which he estimated at less than a mile. The results which may be expected at the now near return of the Leonids will be of especial interest, but it will be for others to utilize them.

Professor Newton was much interested in the collection of meteorites, and the fine collection of stones and irons in the Peabody Museum of Yale University owes much to his efforts in this direction.

Professor Newton was a member of the American Metrological Society from the first, and was conspicuously active in the agitation which resulted in the enactment of the law of 1866, legalizing the use of the metric system. He prepared the table of the metric equivalents of the customary units of weights and measures which was incorporated in the act, and by which the relations of the fundamental units were defined. But he did not stop here. Appreciating the weakness of legislative enactment compared with popular sentiment, and feeling that the real battle was to be won in familiarizing the people with the metric system, he took pains to interest the makers of scales and rulers and other devices for measurement in adopting the units and graduations of the metric system, and to have the proper tables introduced into school arithmetics.

He was also an active member of the Connecticut Academy of Arts and Sciences, serving several years both as secretary and president,—also as member of the council. He was associate editor of this Journal from 1864, having especial charge of the department of astronomy. His notes on observations of meteors and on the progress of meteoric science, often very brief, sometimes more extended, but always well considered, were especially valuable.

In spite of his studious tastes and love of a quiet life, he did not shirk the duties of citizenship, serving a term as alderman in the city council, being elected, we may observe, in a ward of politics strongly opposed to his own.

Professor Newton married, April 14th, 1859, Anna C., daughter of the Rev. Joseph C. Stiles, D. D., of Georgia, at one time pastor of the Mercer Street Presbyterian Church in New York City, and subsequently of the South Church in New Haven. She survived her husband but three months, leaving two daughters.

In all these relations of life, the subject of this sketch exhibited the same traits of character which are seen in his published papers, the same modesty, the same conscientiousness, the same devotion to high ideals. His life was the quiet life of the scholar, ennobled by the unselfish aims of the Christian gentleman; his memory will be cherished by many friends; and so long as astronomers, while they watch the return of the Leonids marking off the passage of the centuries, shall care to turn the earlier pages of this branch of astronomy, his name will have an honorable place in the history of the science.

J. WILLARD GIBBS.

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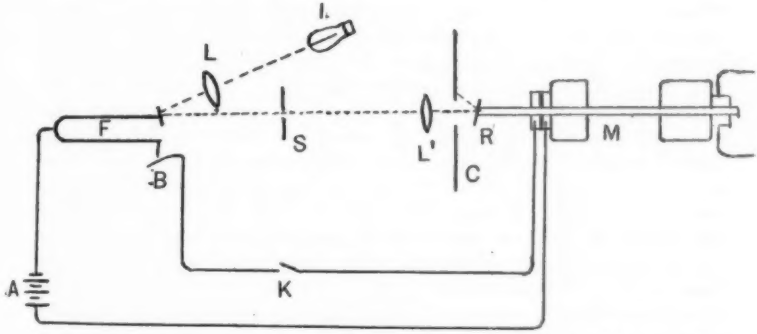
Art. XXXV.—*On a means of producing a Constant Angular velocity;* by A. G. Webster, Ph.D.

In many physical determinations one of the most important elements, and one of the most difficult to maintain constant, is a velocity of rotation. If the velocity is very small, and very little power is required to be transmitted, a clock-work device with some form of escapement is all that could be desired. If as much power is needed as is required to drive a large telescope or siderostat, however, the clockwork becomes expensive, and various governing devices other than escapements have to be employed, and the very great number of such devices actually in use shows how unsatisfactory the method must be. In electrical determinations in absolute measure, such as determinations of the ohm or of "*v*," clockwork is out of the question, as a high speed has to be kept up, and resort has generally been had to water or electrical motors, the speed being



governed by a special observer, who compared the angular velocity with the frequency of a tuning fork by some stroboscopic method. It has doubtless occurred to many that it would be possible to regulate an electric motor by an intermittent current interrupted directly by a tuning-fork, and such an arrangement, proposed by Marcel Deprez, is quoted as one of the earliest forms of synchronous motor. I am not aware whether the experiment was ever actually carried out; in any case no one seems to have made use of the device in practical cases where any amount of power was needed. At the Electrical Congress in Chicago in 1893 I read a short note on an arrangement that I had used, but as I have had a number of inquiries on the subject I think it may be worth while to publish the matter in a scientific journal.

Upon the shaft of a continuous current motor, M, are carried two armatures, one for the main driving current, the other, consisting of a continuous winding with collecting rings, for the auxiliary regulating current. In the motor generally used by the writer for this purpose, and intended for about one horse-power, the two armature windings are wound one over the other, and are in the same field. This is more compact than if two motors are connected together, and somewhat more convenient than if the second armature is replaced by ring connections at two points in the main armature, which is simpler. F is the controlling tuning fork, independently driven by a single storage cell. The auxiliary current, furnished by the storage-battery A, is led into the tuning-fork, through a platinum wire carried on one prong to a mercury break B, described in the next article, and then through a switch K to the brushes of the auxiliary armature. The synchronism is attained by means of the following arrangement. A mirror carried on one prong of the fork forms, by means of a lens L, an image of the filament of an incandescent lamp I, whose plane is horizontal, on a screen S. This screen has a narrow horizontal slit, which allows the beam of light to pass when the fork is nearly, but not exactly, in its equilibrium position. The beam then passes through a hole in a screen C, and being reflected from the mirror R, set obliquely on the shaft of the motor, forms by means of the lens L′ a small bright image on the other side of the screen C. If the fork is at rest and the motor revolving, of course this spot is drawn out into a bright circle. If now the fork be set in motion, the beam of light is interrupted, and the circle is broken up into a large number of bright arcs, which revolve in one direction or the other, according to circumstances. As the speed of the motor increases, the number of arcs is successively reduced, until finally there are only two, when synchronism is nearly attained. The two arcs move first in the reverse direction to that of the motor, and as the speed increases, finally stand still, and then begin to move in the direct sense. By regulating the resistance in the field magnets of the motor, the spots may be made to stand still. We then have a convenient stroboscopic method suitable for regulation by hand in the ordinary manner. If at this point the auxiliary current is thrown on by the switch K, the regulation goes on of itself, and if the auxiliary current has been made of the proper strength, without further attention. Any fluctuations are shown by the oscillations of the two arcs about their mean positions, and by measuring the angle of oscillation and the time in which it is performed, the variation from constancy of the angular velocity may be determined. Of course the *mean* angular velocity

is constant to the limit of constancy of the frequency of the tuning-fork. The observer, being released from governing the rotation, is at liberty to determine this limit of constancy by comparing the fork with a standard by Lissajous's method. In this manner I found that it was not difficult to obtain an accuracy for the fork approaching one part in twenty thousand. The oscillation of the motor about this mean may be made, by means of a fly-wheel, very small.

In throwing on the auxiliary current it is of course necessary for the motor to have attained not only the proper velocity, but the proper phase. In order to determine this it is necessary to distinguish between the two bright spots, and this is the object of placing the slit S in an unsymmetrical position with respect to the vibration, so that the two arcs are not opposite each other. The proper position of the arcs for throwing on the current is determined by a few trials, and may be marked on the screen. In order to warn the observer of a violent oscillation, if he is not looking at the screen, a telephone may be placed in shunt with the brushes of the auxiliary armature, and will, if the synchronism fails, by its loud beats call the observer even from the next room. I believe a device of this sort has been patented by one of the great companies for the purpose of indicating the phase of alternators to be run in parallel—I presume it is possible to patent the use of a telephone for waking one in the morning, or anything else, but such an arrangement appears trivial,—it was used by me more than four years ago.

In order to test the method practically by the transmission of some power, two years ago I belted to the pulley of the motor a heavy cone-pulley running nicely on points, and that to the cone-pulley of a lathe of twelve inches swing. The lathe had a heavy fly-wheel for foot-power, which was belted to the pulley to take up the power and furnish a means of checking oscillations. A cord running over the intermediate cone pulley and carrying a number of weights furnished the means of absorbing most of the power. The motor was run by means of an Edison dynamo designed for 110 volts, but by means of resistance in the field the voltage was brought down, so that of course the regulation of the potential was not very good. The results therefore exhibit the method at its worst, while driving from a storage battery would give far better results. The frequency of the fork used was twenty-five per second. The gearing ran the lathe at at least twice its ordinary speed, the large treadle-pulley making 3·8 revolutions per second, and causing considerable vibration. In the first run the armature had 47 volts at the brushes, and 12 ampères. The governing current was then 1·9 ampères (mean intermittent).

The run of five minutes was reasonably steady, the oscillations being slow, and the greatest oscillation of the spot in the whole period being not over seventy degrees. Later the oscillations became worse, and then a complete to and fro oscillation of forty-five degrees on each side of the mean took place in eight seconds, corresponding to a maximum variation from the mean of one part in eight hundred. These were about the worst figures obtained. The next run of twenty-three minutes maintained the synchronism until the voltage had dropped from 46·5 to 45 volts. Further runs were,

Volts, 53·5. Ampères, 14·5. Auxiliary current—Ampères, 6·2.

The run lasted half an hour and stopped only on account of the failure of the pump belonging to the break (see below). In this time the voltage had gone up one volt, and the ampères one-half ampère.

Volts, 69. Ampères, 19·4. Auxiliary current—Ampères, 6.

Run of fifteen minutes. Oscillation very slight, even with variation of two volts and a quarter and one quarter ampère in the supply.

Volts, 97. Ampères, 12·5. Auxiliary current—Ampères, 4·25.

When the synchronism failed it was on account of the large change in load caused by slipping of the driving belt.

It is thus evident that the method is a practical one up to more than one and a half horse-power. This is more than would be needed in by far the greater number of its obvious applications. The method seems to me of undoubted usefulness in absolute determinations, and may be useful in connection with the driving of telescopes. It is difficult to get exact statements as to the accuracy of existing governors, so that I am unable to state whether the method here given is sufficiently accurate or not. It has been used in the laboratory of Clark University for a number of purposes, one of which is described by Mr. C. A. Saunders in an article in the Physical Review, Sept.–Oct., 1896. By the same means a siren is converted into an instrument of precision, far superior to apparatus costing much more. A chronograph for astronomical purposes has been constructed on the same principle, whose performance will be described elsewhere.

Art. XXXVI.—*A Rapid Break for large Currents;* by A. G. Webster, Ph.D.

The necessity frequently arises of breaking a current of a number of ampères with regularity and considerable rapidity, particularly in connection with induction coils, as in the experiments of Hertz, Tesla, and recently of Röntgen. The breaks usually furnished with induction coils are useless for much more than five ampères, and are at best slow and irregular. Mercury breaks are usually very disagreeable to work with and soon become useless from oxidation. Perhaps as simple and satisfactory a break as any is a revolving slate disc with metallic sectors, as used by Wadsworth, Pupin, and others, with, if necessary, an arrangement for blowing out the spark. Such breaks, however, usually burn up in time. If a very regular break is necessary, however, this device is not applicable, and one naturally has recourse to a tuning-fork. Such a necessity presented itself to the writer when he attempted to regulate the speed of an electric motor as described in the preceding article. With a tuning-fork a mercury break becomes a necessity, as anything else would produce too great a disturbance in the period of the fork. In order to prevent burning up the mercury the contact must take place under some fluid. Water is dirty, alcohol takes fire, and other liquids generally produce a great amount of refuse in a short time. It is therefore necessary to keep a current of liquid flowing over the mercury. After trying a stream of water for some time, and finding that the mercury gradually disappeared from oxidation, and that the platinum wire was also consumed, the writer was laying the matter aside when he was informed by Professor Wadsworth, of the University of Chicago, that a stream of mercury from a jet was used in the laboratory there, for currents of one or two ampères, the idea having been suggested, I believe, by Professor Stratton of that university. A jet of water had been previously used by Bedell and Crehore. It occurred to the writer that if the break were made under water, and the mercury were continuously elevated, that this would furnish the desired break.

The first thing that suggested itself as a means of elevating the mercury was an application of the ingenious device described by Dr. Pupin, in connection with a Sprengel air-pump (this Journal, January, 1895). A short experience with a vertical mercury-jet surrounded by a stream of water showed that this arrangement would not answer, on account of spattering of both water and mercury. The arrangement finally

adopted is shown in fig. 1, the elevator being on the left. An aspirating water-pump is connected to the tube C, at the end

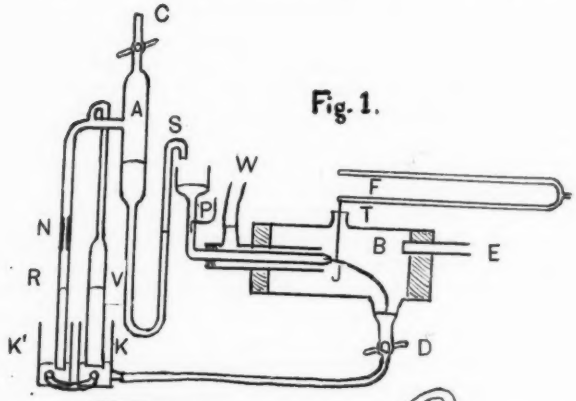


Fig. 1.

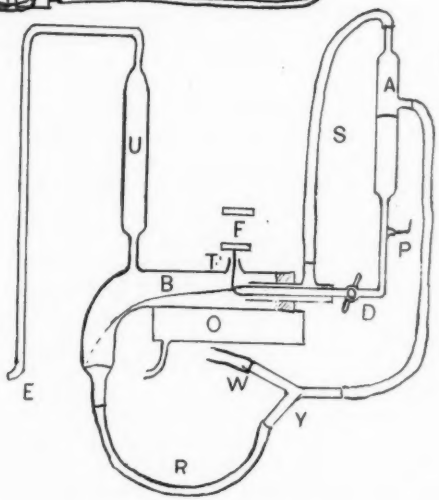


Fig. 2.

of the supply-tube A, which contains mercury at the start. On account of the vacuum, the mercury to be elevated, contained in the cups K and K′, which are connected by a small rubber tube, rises in the tubes R and V, while the mercury in the discharge tube S descends. Owing to the large size of the tube V, the level in K descends rapidly, followed by that in K′, so that the end of the tube R is exposed to the air, which raises the column of mercury in R. When the column passes the con-

striction N, it is suddenly jerked up over into A, the level in S rises, and the quantity corresponding to that which has been raised runs over into the funnel. The mercury in V has in the mean time run down into K, and the process is repeated. The mercury in the funnel runs down through the jet J, through the tube B full of water, runs out at the bottom through the cock D, and through a rubber tube descends to the cup K. The current is introduced by a platinum wire sealed into the glass at P, and goes out at the wire carried by the fork F, and dipping into the mercury through the short standing-tube T. Water is introduced at W around the jet, and flows out at E. The tube is kept full of water, so that the jet is kept cool; the jet presents a continually fresh surface of mercury, the mercury is washed in the flowing water, and comes out perfectly clean. The oxide goes off in the waste water at E. The surface of the mercury is kept just below the tube B, so that no water goes over into the elevator. The cock D is for the purpose of keeping the mercury from all running down into the cups K on stopping the action of the apparatus, and C is to prevent water from the pump getting into A on stopping the aspirator.

The apparatus thus described was very satisfactory, a test made of its automatic nature having shown that a current of twelve mean ampères was carried for an hour without any adjustment of the apparatus. For longer periods it would be necessary to occasionally pour in a little mercury to replace that lost by oxidation, much of which may be recovered by running the waste water through a pail in which it may settle. The above arrangement seeming rather unnecessarily complicated, it occurred to me to try to raise the mercury directly by water instead of air pressure. To this end a jet of water was introduced into a tube containing the mercury to be raised, somewhat in the manner of the Giffard injector. This was successful, but the mercury was carried up in a multitude of fine globules, which sometimes made the stream discontinuous. The arrangement was finally simplified by my assistant, Mr. A. P. Wills, into a simple Y-tube, and the apparatus as now used is shown in fig. 2. The water is introduced at W into one arm of the tube Y, into which the mercury enters through the other branch from R. The mercury is carried to the right and upwards, and is thrown into the supply tube A in quantities of about a tea-spoonful at a time. The water escapes at the top of A and descending through the rubber tube S enters around the jet and keeps the break cool. The waste water rises through the wide tube U, so that any mercury that might be carried over settles and falls back. The water escapes through the siphon E, which should have a pinch-cock at the end to

prevent air being sucked in at T on stopping the action. The cock D is also to be closed at the same time. O is a trough to catch any water that may overflow at T. The flow should be adjusted so that water is just about to overflow at T; the spark is then well under water, and no air is likely to be drawn into the water tube P. The tube R must have length enough to hang down with a certain amount of slack, so that the mercury shall have inertia enough for the proper working of the pump. When the proper arrangement is arrived at, the level of the mercury fluctuates with a regular pulsating motion, and at each pulsation the water is momentarily shut off in Y and mercury is carried up. In order to start the apparatus working it is only necessary to turn on the water and open the cock D. The apparatus of the size indicated in the figure working on a head of two and one-half meters of water with a flow of about three-tenths of a gallon per minute, has carried a current of twenty ampères, and has been used with forks giving fifty and one hundred breaks per second. I know of no other arrangement which will stand such a current for any length of time.

Clark University, Worcester, Mass.

Art. XXXVII.—*The Electrical Conductivity of the Ether;* by John Trowbridge.

I stated in the April number of this Journal that my experiments have led me to the conclusion that the chief resistance in overcoming the apparent resistance of a highly rarified medium is encountered at the surface of the electrodes (*ueber gangschicht*) and that when this is overcome the ether offers little resistance. The method I have employed seems to me to be a very useful one for the study of electrical discharges. It may be termed the damping of the additional spark method, or the comparison of resistances by the estimation of the damping of electrical oscillations.* The electrical circuit is provided with two spark gaps. One of these is placed in a gas, or under the conditions which are to be examined, while the other is photographed according to Feddersen's method by a revolving mirror. With cadmium terminals this method enables one to estimate the resistance of a spark in air or in rarified media to one-half an ohm.

Having at my command a battery giving a voltage of twenty thousand, with an internal resistance of only one-quarter of an ohm per cell, and capable therefore of giving a very powerful current, I first studied the behavior of Crookes tubes which were connected to the terminals of this battery. I found that no Röntgen rays could be obtained with a voltage of twenty thousand. On heating the Crookes tubes, they were filled with a pale white light, which showed very faint bands in the green when examined by the spectroscope. Then the entire strength of the battery appeared to be manifested in the tubes, the electrodes became red hot—the medium broke down and offered no resistance to the current of the battery. This white discharge showed even at its culminating point no Röntgen rays. I then employed the Planté rheostatic machine. This apparatus, I think, has not received sufficient attention from physicists. In connection with a large battery, it is very efficient, and it enables one to form an estimate of the high electromotive force that one employs in the study of the Röntgen rays. I have slightly modified the form of the machine as it is given by Planté. The main principle consists in discharging Leyden jars in multiple and then discharging them in series. The proportion of the length of spark to the number of jars is very close. Knowing the electromotive force of the battery which charges the jars, we can estimate the voltage necessary to produce sparks of different lengths. I

* Damping of Electrical Oscillations on iron wires, Phil. Mag., Dec., 1891. Also American Journal of Science, April, 1897.

speedily found that at least one hundred thousand volts were necessary to produce the Röntgen rays and they were produced more intensely as I increased the voltage, certainly to the point of five hundred thousand volts.

In order to ascertain whether the discharges through the Crookes tubes when the Röntgen rays were apparently produced most strongly were oscillatory, I first placed a Geissler tube in the circuit with the Crookes tube and carefully observed the appearance of the two electrodes of the Geissler tube. They were quite alike and indicated an oscillatory discharge. I then replaced the latter tube by a small spark gap and photographed it in a rapidly revolving mirror. The photograph showed at least ten oscillations with a period of about one ten-millionth of a second with the Crookes tube and the circuit I employed. Furthermore, applying the method of estimating resistances by the method of damping, I found that the resistance of the rarified medium was less than five ohms. The energy, therefore, at the moment of the emission of the Röntgen rays, was not far from three million horse power, acting for one millionth of a second. I employed also a Crookes tube with an aluminum mirror of about two centimeters focus. The resistance of this tube to the discharge was the same as that in which the mirror had a focal length of six centimeters. Incidentally, there seems to be no advantage in shortening the distance between the cathode and the anode—by employing a mirror of short focus. Struck by the fact that the distance between the electrodes did not appear to make any appreciable difference in the resistance of the Crookes tube, I replaced the latter by a spark gap of six inches in length in air, and photographed the spark in another gap in air in the same circuit. This latter gap was one-quarter of an inch. The photographs showed on an average the same number of oscillations whether the additional spark gap was six inches in length or one inch in length. I found, moreover, that on increasing the electromotive force the resistance of the sparks in air decreased. By quickly drawing apart the terminals of my large battery I can produce a flaming discharge in air of about three feet in length. Rhigi has also observed the same phenomenon with sparks from an electrical machine. We see that no increase in resistance results. I then placed the secondary spark gap in a receiver and studied the resistance offered by rarified air at the point when long ribbon light white disruptive discharges can be obtained. This point is at about 100^{cm} pressure. The resistance of such discharges of about six inches in length in a receiver containing air at this pressure is two or three ohms more than sparks of one quarter of an inch in air, which have a resistance of from 2 to 3 ohms. On measuring by the above

method the resistance of sparks of different lengths in the receiver at this pressure, no difference in resistance could be perceived between a spark of six inches in length and one of three inches in length.

The secondary spark gap was next placed in a chamber of air which was compressed to four atmospheres. This amount of compression made no difference in the resistance to the disruptive discharges. The secondary spark was also obtained in hydrogen gas generated by electrolysis at atmospheric pressure, and no appreciable difference in resistance between this gas and air was noticed. The length of spark which could be obtained with a given voltage was somewhat more in hydrogen than in air. It was interesting, in the next place, to determine by this method whether differences in the material of the spark gaps made any difference in the resistances observed in the case of disruptive discharges.* I accordingly employed terminals of platinum, iron, aluminum, brass, cadmium, and zinc, and could perceive no difference. Moreover, any difference of resistance between spheres and between pointed terminals, or between a point and a plane, seemed to be inappreciable. With powerful discharges such differences, if they exist, apparently disappear. The secondary spark was next placed in a heated flame. It is well known that the spark length can be thus greatly increased. On photographing a spark in an additional gap, the resistance appeared to be slightly increased in the flame; doubling the length of this spark however, made no change in the resistance that was encountered in the heated medium. The phenomenon was exactly analogous to that observed in the receiver exhausted to 100^{cm}. I was interested to observe whether heating the spark in the primary of a Thomson Tesla transformer produced any marked change in the high tension spark of its secondary. It was evident that it was detrimental. The high tension sparks immediately ceased to jump at the extreme sparking distance of the terminals. Following this train of thought, I next placed a spark gap of the primary of the above mentioned transformer between the poles of a very powerful magnet, giving a field of certainly ten thousand lines to the centimeter. It is well known that when such a field is excited, the primary spark appears to be blown out with a loud report and a great increase of length of spark is obtained in the secondary of the transformer. Applying the same method, I photographed the spark of the additional spark gap and found no difference in resistance whether the magnetic field was excited or not: or when the spark jumped across the magnetic lines or in the

* Rhigi ovo Cimento (2), 16, p. 97, 1876; De la Rue and Hugo Muller, Phil. Trans., 169, Pt. 1, p. 93, 1878.

direction of the latter. Is it possible that, the ether being already under a magnetic stress, the addition of a powerful electrostatic stress serves to suddenly break down the ether? It is well known that a blast of air imitates the action of the magnetic field. It probably does so by blowing out the voltaic arc which tends to form. It may be that the electrodynamic repulsion compels the spark not to follow, so to speak, the voltaic arc and its current of heated air. The loud report may indicate a sudden stress in the medium, and in the case of the Crookes tube the highly rarified medium within it would effectually prevent our hearing a similar report.

I next placed the primary spark of the Thomson Tesla transformer near a Crookes tube which was giving out the Röntgen rays. I could not perceive any mutual effect. The effect, moreover, of ultra violet light on the resistance of sparks in air could not be detected.

The method I have outlined enables one to form an estimate of the energy incident upon the production of the Röntgen rays. It can also measure with greater accuracy the resistance of sparks in air and different media. It shows conclusively that the discharge in a Crookes tube at the instant when the Röntgen rays are being emitted most intensely, is an oscillatory discharge. In popular language it can be maintained that a discharge of lightning a mile long under certain conditions encounters no more resistance during its oscillations than one of a foot in length. In other words Ohm's law does not hold for electric sparks in air or gases. Disruptive discharges in gases and in air appear to be of the nature of voltaic arcs. Each oscillation can be considered as forming an arc. It is well known that a minute spark precedes the formation of the voltaic arc in air. The medium is first broken down and then the arc follows. I believe that this process occurs also in a vacuum and that absolute contact is not necessary to start the arc. My experiments lead me to conclude that under very high electrical stress the ether breaks down and becomes a good conductor.

Jefferson Physical Laboratory, Harvard University.

ART. XXXVIII.—*The Effect of Great Current Strength on the Conductivity of Electrolytes;* by THEODORE WILLIAM RICHARDS and JOHN TROWBRIDGE.

IN a recent paper on the temperature and ohmic resistance of gases during the oscillatory electric discharge,* we have described a method of determining resistance by measuring its damping effect upon electric oscillations. The method is obviously one which will apply to electrolytes also, provided that the resistance to be measured is less than twenty ohms; and it seemed to be very well worth while to determine if the intense current involved in the discharge of a large condenser is capable of causing any change in the condition of an electrolyte.

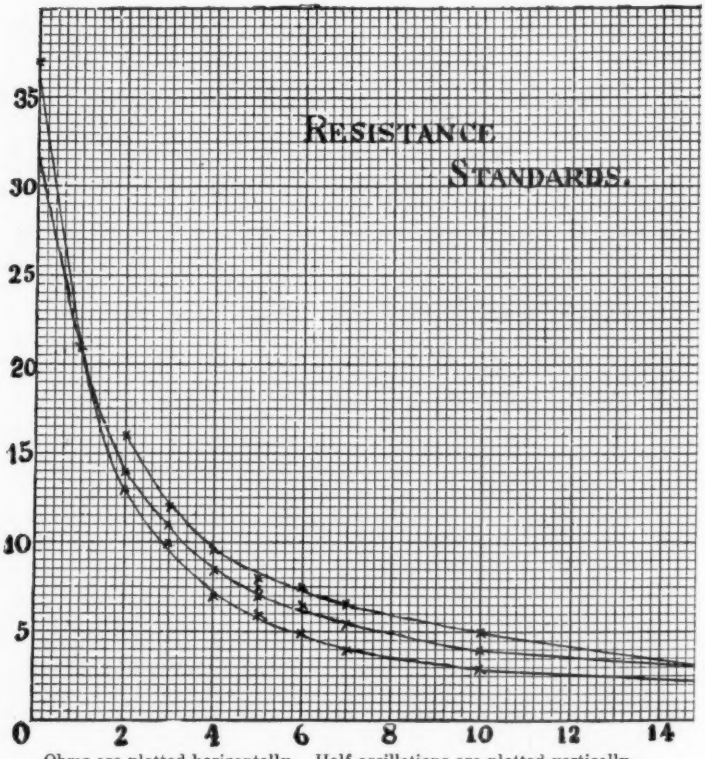
In our first experiment, two copper plates of sixteen square centimeters area were clamped at a distance of three centimeters apart by means of vulcanite. Upon being immersed in a saturated solution of pure cupric sulphate at 15° C, the plates allowed about ten oscillations from one of our large Leyden jars, nine from two jars, and eight from three jars, to pass through it. According to the scale of standards, reprinted from our last paper,* each of these results corresponds to a little less than four ohms resistance. By means of Kohlrausch's method, using a very small inductorium, this cell gave an extremely poor minimum at a point corresponding to a resistance of about ten ohms. The plates, which had purposely been left very dirty, in order to test the efficiency of the method, were now scrupulously cleaned with alkali and acid, and were then both carefully plated with pure copper. With Kohlrausch's method, the cell now gave an excellent minimum at exactly four ohms resistance, and further cleaning and plating caused no further change. New photographs of the sparks from the two jars sent through the cell showed again about nine half oscillations, corresponding to about 3·8 ohms. It is evident, then, that the resistance of concentrated cupric sulphate is not essentially altered by great alterations in the strength of the current.

Experiments with zincic sulphate gave similar results, and a solution of cadmic sulphate between cadmium electrodes which possessed a resistance of 4·7 ohms according to Kohlrausch's method, gave nine, seven, and six half oscillations with one, two, and three jars respectively, corresponding to about 5 ohms in each case.

Undoubtedly the reason why the strong instantaneous current, which alters so much the resistance of gases, has so little effect upon solutions, is because of the great mass and specific

* This Journal, vol. iii, p. 327.

heat of the material which must be warmed in the latter case. The average temperature of the solution rose during our experiments only at a rate of about 1° in three minutes.



Ohms are plotted horizontally. Half oscillations are plotted vertically.

A similar, although smaller, heat capacity prevents the wire resistances which are used as standards from becoming seriously altered in resistance by the heat. We had used manganin wire in our tests, but in order to be sure that our fine short wires had not been overheated, we constructed a five-ohm resistance of four strands of coarser manganin wire about 0·25^{mm} diameter and 3·5 meters long. This was stretched upon each side of a thin vulcanite plate to avoid self-induction, but it allowed essentially the same number of oscillations to pass as did the short fine wire. A short German-silver wire, with a very high temperature-coefficient, showed a conductivity only a very little

less; thus the error from the heating of the wire may be neglected.

In order to show that common electrolytic polarization does not interfere with the accuracy of our method, we measured with the help of our 20,000 volt storage battery and condensers the resistance between two bright platinum plates similar in size to the copper ones used before, in a cupric sulphate solution. This was found to be four ohms; and after plating the electrodes with copper the resistance remained unchanged. Kohlrausch's method gave no satisfactory result with both electrodes free from copper; but when both were plated it indicated a resistance of 3·9 ohms.

Our method may therefore be a useful one for the approximate determination of conductivities in cases where impurities or polarization render Kohlrausch's method unsatisfactory. For accuracy, of course, pains must be taken to develop all the photographs in the same fashion, and in general to arrange the conditions of the exposure alike in all cases.

Our conclusion, that the conductivity of electrolytes is not essentially affected by great changes in current strength, only emphasizes all the more strongly the conclusion of our last paper, that the conductivity of gases is very much affected by changes in the current strength.

Harvard University, March 8, 1897.

Art. XXXIX.—*On the Southern Devonian formations;* by Henry S. Williams.

[Read before the Geological Society of America, Dec. 31st, 1896.]

One of the most remarkable contrasts of geological correlation is met with on passing from the Devonian system, as it is typically expressed in New York state, to the expression of the same system as found in Tennessee and Alabama.

In the north, following the Silurian formations in regular sequence, there is a series of varying sediments, reaching several thousand feet in thickness, made up of numerous separate formations, sharply differing in the kind of materials composing them and containing well-differentiated faunas. In the south a uniform black shale, with perfectly even sedimentation, the grain of the rock presenting almost no difference from top to bottom, containing a meagre fauna and evidence of a common plant vegetation so long and so far as it may be distributed in its purity, and varying in thickness from 500 to 900 feet in some sections to a few inches in thickness in some of the Alabama sections, and often resting unconformably upon Niagara,

Trenton or even lower rocks. The remarkable nature of this contrast is emphasized by the fact that the two extremes are both within a common intercontinental basin.

In order to understand the nature of the problem it is essential to consider the geographical conditions which prevailed during the Devonian era in this eastern part of North America. Referring to the chart on the opposite page, it will be observed that the main part of the eastern half of the continent south of the great lakes and west of the ridge of the Appalachians was a Mediterranean sea. The northern shore extended from Minnesota to eastern New York. The land to the north (L), which was chiefly Archæan, may be called Laurentia. On the east (A) was a shore line extending from eastern New York to central Alabama, the land of which may be called Appalachia. Indications in Arkansas, Indian territory and Texas point to land surfaces along the gulf-border states (K, T), but their exact extent, and whether islands or continuous shores on the southern border of the sea, is uncertain. From the north a wide open oceanic channel swept from the Mackenzie river valley region across British America, the Dakotas, Nebraska, and Kansas far into and through the western Texas region to the south. This channel was bounded on the west by the extensive Archæan islands or edges of lands constituting the eastern axis of the present Rocky Mountains (R), and it may be called the Dakota channel.

From the northern shore of the interior continental sea, over what is now Wisconsin, a peninsula (W), extended southward in the early part and, perhaps, the closing part of Devonian time, which may have constituted shallows as far as the Missouri island (M). This Missouri island occupied considerable of the southeastern part of Missouri, made to be of greater or lesser extent with the oscillations of level which occurred during the Devonian era. In the midst of the intercontinental sea thus constituted there was a low ridge (C), sometimes land, sometimes partly land and partly shallow ridge, under tide level, which extended more or less continuously from western Ontario to central Alabama; this may be called the Cincinnati plateau. The rocks forming the surface of this plateau were, chiefly, Ordovician limestones, while the shores of Appalachia and Laurentia were chiefly of Archæan rocks.

In studying the Devonian system of this interior continental area most of the differences in stratigraphy, met with along the northern border and the northern part of the Appalachian shore, are readily interpreted by the application of two general geological principles. These are the following: (1) the differences between limestones on the one hand and clastic rocks, composed of fragments of argillaceous, arenaceous or conglomerate nature, are accounted for by the different origins of the

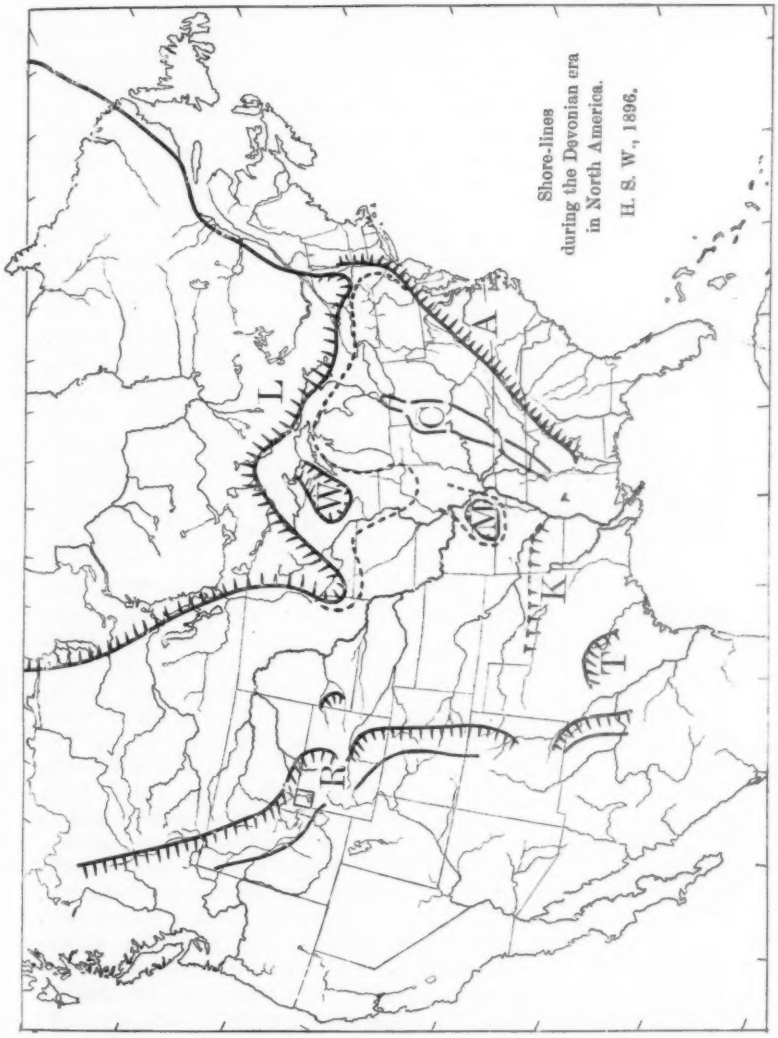


Chart showing the approximate position of the Devonian intercontinental sea. The strong lines with fringes represent the edges of undoubted land masses of Silurian time; the dotted lines probable extension of these shores during Devonian time; heavy line about C = oscillating shore lines of which the exact margin is unknown; L, Laurentia land; A, Appalachia land; R, Rocky Mountain lands or islands; W, Wisconsin peninsula; C, Cincinnati plateau; M, Missouri island; K, probable edge of a land-mass over Arkansas and Indian territory; T, a piece of shore-line in Texas, the extension of which is unknown.

materials themselves; i. e. limestone, carbonaceous and phosphatic materials are of organic origin, while clastic sediments are derived from the decay, fracture and grinding up of preexisting, generally, crystalline and, therefore chiefly, Archæan rocks. (2) The differences in size or fineness of grain of the materials of clastic rocks may be accounted for by the sorting action of moving waters; and thus all the varieties of rocks are brought into a general relationship to the distance from the shore-line off which they were finally deposited. By the application of these general principles it was found possible to interpret the geographical changes in the stratigraphy of the formations across New York state, northern Pennsylvania and Ohio in terms of distance from the shore line and of oscillations of that line backward and forward with general vertical movements of the crust. By these principles the Catskill was shown to be, not a definite chronological formation representing the time following the period of the Chemung fauna, but an along-shore facies of the Devonian deposits of the northeastern bay of this interior sea incident to a gradual rising of the lands at that part of the region. The sediments of Catskill facies, therefore, occurred as early as the Hamilton in eastern New York, while they did not occupy the area of western New York till after the end of the Chemung period.*

The entrance of the Cuboides fauna into the New York area east of the Cincinnati plateau was also thus found to be coincident with the changes of level which were recorded in central-western New York and Pennsylvania, by the presence of a local limestone of the Tully epoch; this depression giving access to the faunas which had already, in Hamilton time, reached as far south as Iowa from the Dakota channel at the north.† While the two general principles named were sufficient to account for these and other differences, I have been unable by any combination or commutation of these alone to arrive at a satisfactory explanation of the Black shales, so conspicuous to the south, or of the Oriskany whose center of distribution seems to be far up in the northeast corner of the sea.

On the supposition that the black shales were clastic sediments, derived from the same sources with the other clastic sediments of the Devonian system, they should appear in the sections, all around the shores of Laurentia and Appalachia, wherever the corresponding fineness of division was reached; but this does not agree with the facts, for in the southern sections black shale sedimentation prevails throughout the whole

* Dual nomenclature in geological classification. Jour. Geol., vol. ii, pp. 145–160, 1894.

† The Cuboides zone and its fauna, Geol. Soc. Am. Bull., vol. i, pp. 481-500. The scope of Paleontology and its value to geologists, Proc. A. A. A. S., vol. xli, pp. 149–170, 1893.

period of the accumulation of these varying sediments, including some sheets of black shale, as well as several belts of equally fine sediments which do not carry the black shale fauna. Also the geographical distribution of the black shales is not at all related to distance from these shores. Hence it has been necessary to give to the geologist, who has appealed to the paleontologist to know what is the age of the southern black shale, the same kind of answer Lonsdale gave regarding the original rocks of Devonshire, that all we know is that they are younger than Silurian and older than the central member of the Mississippian series of the interior. In like manner the sudden appearance of the coarse and quite uniform sands of the Oriskany epoch, in the northeastern corner of the interior continental sea, following immediately after the Helderberg, which was a limestone with its center of distribution also there, finds no solution upon the theory of rise of the shore, especially when we consider that its distribution is limited to the shores of the Laurentia and Appalachia lands and possibly some parts of the Cincinnati plateau. Some other factors than those usually applied to the solution of correlation problems must be brought under consideration. It is necessary to explain the general conditions of sedimentation for the whole basin in Devonian time, and also the reasons for the differences of sedimentation in different parts of the area. It is also necessary to explain the almost total absence of a series of varied faunas, abundant in the northern and northeastern part of the basin, from the southern central part. To reach these ends a minute study of the relations of the black shale to the sediments and conditions both preceding and following its occurrence, in purity, seemed likely to throw some light. The studies already made in Arkansas and Tennessee seemed to warrant the conclusion that in certain sections of the south there was unconformity below the black shale, and the suspicion was forced upon my mind that the base of the shales themselves was of varying age. The suggestion made in Mr. Hayes' paper on the Tennessee phosphates,* that the bottom might have been scoured by ocean currents, led me to think that ocean currents might account for the distribution of the black muds.

In order to study the facts more minutely I spent several weeks last summer in southern Virginia, Tennessee and Kentucky. The Estillville sheet† of southern Virginia prepared by Mr. Campbell offered the most promising field for ascertaining the relationship of the black mud sediments to other clastic

* The Tennessee Phosphates, by Charles W. Hayes, 17th Ann. Rept. U. S. Geol. Survey. Part IV. pp 610–630, 1895.

† Estillville Folio. Ky. Va. and Tenn. U. S G. S. Geol., by M. R. Campbell, 1894. "Geology of the Big Stone Gap Coal field of Va. and Ky.," by M. R. Campbell. Bull. No. 111, U. S. G. S., 1893.

types, because of the great thickness of the black "Chattanooga shales" at Big Stone Gap and the great thickness of the following "Grainger shales" which were called Devonian, though with some hesitation, as Mr. Campbell stated, on account of lack of fossils. An examination of the black shale at Big Stone Gap, which is at least 500 feet thick, revealed not the least addition to the meagre Lingula fauna, but made clear that the following arenaceous shales and sandstones began as very thin intercalated sheets, thin as paper at first, far down in what, to the casual observer, appeared to be pure black shale. This led to the expectation that the section farther east (i. e. nearer to the ancient shore line) would show the shore-derived sediments earlier in the section. This expectation was confirmed by following the section eastward to Moccasin Gap, where in the arenaceous shales well above the pure black shale, in what was mapped as Grainger shale, a distinct Carboniferous fauna (the *Syringothyris* fauna of the Kinderhook age) was discovered. These observations confirmed the belief that the black shale material was derived from a different direction from the other clastic sediments making up the section.

Examination of the sections at Irvine, Kentucky, the other side of the Cumberland channel (the name which may be given to the southern part of the Devonian intercontinental sea lying between the Cincinnati plateau and Appalachia) revealed the fact that there the black shales were thinner but held on in their purity well up into Carboniferous time. The intercalations consist of calcareous and ferruginous, concretionary sheets and carry undoubted Carboniferous fossils and occur in the sections before the black shale loses its characteristic expression. This settled the suspicion that the black shale materials were derived from the direction of the Cincinnati plateau and not from the Appalachia side of the channel, since on the plateau side of the channel they were pure and continued later in the section, and on the Appalachia side, where there were other clastic sediments following, these sediments from the Archæan land-wash increased and were found lower down in the section on approaching the land edge.

Thus the conclusion was reached that the Cincinnati plateau was the source of the black shale muds, and to account for their distribution the agency of an ocean current, such as the Florida current of the Gulf stream is at present in the Atlantic, was introduced.

The next question arising was as to the age of the base of the black shale. The study of the Big Stone Gap sections also threw light on this problem. On the Kentucky side of the Cumberland channel, as well as on the Appalachia side, there are frequently represented at the base of the black shales beds of brown iron ore. About Big Stone Gap this brown ore con-

tains corals which appear to be of Corniferous age, and below is a sandstone capping the Helderberg limestone. At a single locality, under the woollen mill on the bank of the east branch of Powell river east of Big Stone Gap, in the place of the ore the limestone is quite filled with corals in place, followed immediately by the black shales with no intervening iron ore. This seems to fix the date of the beginning of the black shales for this region at an horizon closely corresponding to that of the Marcellus shale in the New York section.

Hence it may be safe to conclude that in Tennessee, Alabama, southern Kentucky and probably Arkansas, whenever the black shales rest on some formation lower than the Lower Helderberg limestone (for the absence of Oriskany will be explained in another way beyond) that either there had been elevation and therefore no sedimentation, or that the rocks had been eroded away before the black shales were laid down. The erosion may have been partly done under water, by the scouring action of a strong ocean current washing in from the west between the Missouri island and the land areas in southern Arkansas (between M and K, see chart). The fact of the interval containing the black shale over much of this region, an interval reaching, often, to the Ordovician and even lower, together with the presence of a low anticlinal of the Cincinnati plateau extending farther north, suggests the probability of land surface more or less continuously connecting the southern end of Appalachia with the Texas area.*

An examination of the chart will at once make evident that in case there was, as we should expect, an ocean current flowing southward through the Dakota channel, the effect of raising the bottom above tide level along the gulf border states (along T and K), would be the deflecting of the current with a powerful swirl into the southern part of the interior continental basin. Even if the elevation were only to make a string of islands in this region, a part of the current would enter the basin and wash across the northern Arkansas, Tennessee, Alabama surfaces, and would tend to move the bulk of the muds of the bottom into the Cumberland channel and thence northward. As a working hypothesis, this is supposed to have taken place; and what we know of the general distribution of the black shales is consistent with such an hypothesis.

The presence of polished sand grains, with nodules of black shale mud, and rolled and polished fragments of large fish bones, some of them from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch in thickness, associated with more or less phosphate of lime, at the bottom of the shales, over the southern extension of the Cincinnati

* In the discussion which followed the reading of this paper Mr. Hayes informed us that the black shale thickens in central Alabama, thus confirming the probability of land in that direction during Devonian time.

plateau, and also along the southern shores of the Missouri island, is further in harmony with the hypothesis of current-scour and transportation. Since the black shale conditions followed soon after the first appearance of the Oriskany sediments and fauna in the basin, this event requires consideration.

The more conspicuous facts regarding the Oriskany crisis are the following: In a great interior continental basin, closed all around its north, east and part of its southern border (see the chart), the closing epoch of the Silurian era was marked by a widely distributed limestone accumulation which appears to have its purer and more characteristic center in the extreme northeastern part of the basin. Here depression was taking place so that in eastern New York the formation is markedly thicker than in its western part. Suddenly there was an irruption of coarse sand with a sharply differentiated fauna, which from Oriskany Falls, N. Y., extends in diminishing thickness across to Ontario, Canada. On the eastern shore, however, along the edge of Appalachia, as far as Georgia, the fauna at its base appears to be more or less blended with the Helderberg species which continue upward in the sections. In the extreme southward extension only a few of the Oriskany species reach the region and the Helderberg fauna does not appear to cease till after the Oriskany conditions of sedimentation had closed. In eastern Pennsylvania, the coarser Oriskany sediments are not found on the extreme eastern side but as far west as the Lehigh Water Gap, where, too, they are thicker, by four times, than at the Delaware Water Gap, some thirty miles further east, and the more eastern sections are limestones and lime shales, while to the westward sandstones and conglomerates appear in heavy sections.*

Furthermore, when we examine the distribution of the fauna we discover that, while it constitutes the principal Devonian marine fauna in the troughs entering from the east into the Acadian and New England region, both the sedimentation and fauna of the Oriskany of the interior continental basin are confined to the shores of Laurentia and Appalacha east of the Cincinnati plateau, and to the eastern shore of the Cincinnati plateau. In its purity the fauna is confined to the northeast bay of this interior continental sea. To explain all these peculiar conditions one event seems to be fully sufficient. If we suppose that the sinking of the area, including the northeastern rim of this basin, continued till sea level was reached, we then have a sufficient cause for the incursion of the vast amount of coarse sands, and with them the new fauna, which suddenly appeared first in the extreme northeast corner of this inland sea. There is sufficient reason for the belief that such a sinking was going on during Helderberg time, and that it extends along the Champlain valley as far as Montreal, near

* See Penn. Geol. Surv. Summary, final report, vol. ii, p. 1042.

which on the island of St. Helena a small patch of Helderberg limestone is still preserved, and still further on to connect with the eastern ocean across the St. Lawrence Valley.

Such a depression is also the natural correlative of the elevation at the southwestern corner of the basin which the various facts already in hand seem to call for. It is not difficult to imagine an event similar to this as possible at the present epoch, by which a slight depression, taking below sea-level the shores of northern Europe, would let in the cold waters of the north to spread over the plains of Russia, into the Black Sea and through to the Mediterranean, changing its fauna, and, if the current were strong enough, covering the bottom with the wash of the land, and with a rapidity, and in quantities, far in excess of the sedimentation ordinarily occurring there. The catastrophic effects would be coördinate with the reaching of a culmination point in slowly and regularly operating movements of the crust. The crisis would occur when the sea level was reached in the slow depression of the northern continental rim. Such an event is supposed to have occurred at the time of the incursion of the great mass of clastic sediments into the Lower Helderberg sea with a new fauna which marked the opening of the Oriskany epoch in North America.

In the hypothesis here offered to explain the differences in stratigraphy presented by the formations of the Devonian in various points of the intercontinental basin of eastern North America, a few fundamental principles are introduced which have heretofore been entirely ignored, or given a very insignificant place in the solution of such problems.

1st. *A two-fold source for the clastic sediments forming the material of the strata of a common section.*—In the present case the black shale sediments are supposed to have been derived from the decay of Lower Silurian rocks protruding more or less above sea-level along the Cincinnati plateau, and not from the surfaces of the Laurentia or Appalachia lands, from which the sediments making up the bulk of the strata bordering these lands seaward were derived.

2d. *The agency of ocean currents in determining the geographical distribution of sediments not derived from the immediate coasts along which they were deposited.*—In the cases of the black shale and of the Oriskany sediments this agency explains conditions of distribution which have not been accounted for by any cause heretofore suggested, so far as I am aware. And the reconstruction of the topographical conditions of the region from facts already known is not inconsistent with the assumption that the required currents actually existed in the basin during the time when the distribution of the sediments took place.

3d. *The catastrophic effects, upon the sedimentation, and upon the inhabitants of a more or less inclosed marine basin, produced by the passing below tide level of a portion of the rim of such a basin.*—In the case of the Oriskany the opening of such a passage from the interior basin to the northeastward, will account for the sudden wash of a great mass of indiscriminate sediments, from the tide-swept, depressed land into the basin during the depression which was marked before and after by limestone-accumulations throughout the northeast bay. This will explain the purity of the Oriskany fauna and sediments only in the northeast corner of the basin, and their entire absence along the western extension of the great basin, from which direction, in the absence of such a depression, we should have expected them to arrive into the waters of the basin.

In addition to these three geological principles there are a few biological principles which have been adopted as working hypotheses, the formulation of which may be appropriate in this connection.

A. It is assumed that in a common intercontinental marine basin, such as evidently existed during Devonian time over this region, the various faunas living in the basin, after having attained a biological equilibrium to their conditions of environment and to each other, will preserve their integrity so long as the conditions of environment remain constant.

B. The passage from one formation to another, involving a change of fauna, is, therefore, assumed to indicate a change in the conditions of environment. This change is usually expressed in terms of difference in the character of the sediments.

C. These changes in the conditions of environment may be of two kinds. One of them consists in oscillations of level, gradually shifting the shore conditions, but consistent with a movement of the species to adjust themselves to the slightly changing conditions. In this way the local differences of a common general fauna may be explained. In this case it is probable that the faunas would not acquire any new genera not already living in the basin, but the readjustment of biological equilibrium might result in the evolution of new varieties and possibly new species.

D. A second kind of environmental change would result when from any cause the relations between the basins as a whole and the ocean outside were changed. Such change might occur by the opening of a new passage to the exterior or by the closure of established passages resulting in deflection of currents of the ocean. But the biological effect would be of a different nature to that in the previous case. The general biological equilibrium would be broken. Not only would new species and genera be introduced, but the breaking of the established equilibrium among the species would result in a specific advance in evolution all around; and we might expect,

occasionally, also new genera to arise as the result of such disturbance.

From these considerations it becomes evident that geological phenomena affecting the topographical conditions of the surface of the earth have, from a biological point of view, varying values.

There have been, in the course of geological time, movements of the earth's crust in one direction, continuously, for sufficiently long periods to lift to mountain proportions the edges of continents. These may properly be called *geological revolutions*, and mark such grand changes as separate the Paleozoic from the Mesozoic, and the Mesozoic from the Tertiary. They probably disturbed the biological equilibrium of the marine faunas throughout the greater part of the earth's surface.

There occurred lesser changes, such as the supposed case of the Oriskany, which disturbed the equilibrium of the faunas within confined provinces, destroying old species, introducing new species, and otherwise inducing modification of the species which survived. These were *geological crises* and were local in their effects.

The lesser changes, resulting in rearranging the local environment along a common shore, or in a common basin, were of less consequence and were of more or less continuous operation, causing the shifting of faunas back and forth with resulting slight changes in the varietal and, possibly, specific character of the species, but chiefly expressed in rearrangement of the dominant and secondary species, in the abundance, and sometimes the presence and absence of species from a fauna whose general features remained intact.

To apply these biological principles to the interpretation of geological events it is proposed that, while occasional modification of forms of specific value may have occurred during the ordinary shifting of local conditions marking different *epochs*, the *crises* which disturbed the biological equilibrium of existing marine faunas were the times when the chief modifications of specific and generic value took place; the interval between two such crises represents a biological *period*. Those changes, marked by the extinction of genera and the introduction of new types of family or higher rank among marine species, were of the nature of *revolutions*, in which vast changes were made in the surface conditions of the earth, particularly in the way of lifting tracts of the ocean bottom to a permanent subaqueous position and in separating the great geological *eras* from one another. Hence in the study of the geological history of the earth the crises, as well as the revolutions, become of prime importance, and in studying the relations of fossil faunas it is important to discover the territory affected by a crisis, and the particular locality where the effect of the crisis was centered.

Art. XL.—*Note on the Genus Lingulepis;* by Charles D. Walcott.

Lingulepis, Hall.

Lingulepis, Hall, 1863. Sixteenth Ann. Rep. N. Y. State Cab. Nat. Hist., p. 129.
Lingulepis, Meek and Hayden, 1864. Pal. Upper Missouri, Pt. I, p. 1.
Lingulepis, Hall, 1867. Trans. Albany Institute, vol. v, p. 106.
Lingulepis, Hall and Clarke, 1892. Eleventh Ann. Rep. State Geologist, New York. (Author's Ed.) p. 231, pl. 1, figs. 16, 17.
Lingulepis, Hall and Clarke, 1892. Pal. New York, vol. viii, Pt. 1, pp. 59, 163.
Type, *Lingula acuminata*, Conrad sp.—*Lingula pinniformis*, Owen.

A comparison of a series of specimens of *Lingulepis acuminata* from the Potsdam terrane and the base of the Calciferous formation of Saratoga, Washington, Franklin, and Jefferson counties, New York, and from the same horizons in Ontario, Canada, with a large series of specimens from the St. Croix sandstone of Wisconsin, leads to the conclusion that *Lingulepis pinniformis* is a synonym of *Lingulepis acuminata*. This makes *Lingulepis acuminata* the type of the genus *Lingulepis*, the original description of the genus being based upon specimens from the St. Croix sandstone of Wisconsin.

The further study of the types of the species that have been referred to the genus *Lingulepis* results in the elimination of all of them from the genus with the exception of *Lingulepis acuminata* and *Lingulepis meeki*.

The species that have been referred to *Lingulepis* heretofore are now referred as follows:

Lingulepis dakotensis, M. and H.—*Lingulepis acuminata.*
Lingulepis pinniformis, Owen.—*Lingulepis acuminata.*
Lingulepis minima, Whitfield.—*Lingulepis acuminata.*
Lingulepis perattenuata, Whitfield.—*Lingulella perattenuata.*
Lingulepis cuneolus, Whitfield.—*Lingulella cuneolus.*
Lingulepis ella, H. and W.—*Lingulella ella.*
Lingulepis matinalis, Hall.—*Lingulella ? matinalis.*
Lingulepis mœra, H. and W.—*Lingulella ? mœra.*
Lingulepis ? minuta, H. and W.—*Obolella ? minuta.*
Lingulepis morsensis, Winchell (Miller)—*Lingula morsii.*
Lingulepis prima, M. and H.—*Dicellomus polita.*

In a review of the Cambrian Brachiopoda, now being prepared, the Cambrian species referred to the genus *Lingulepis* will be fully illustrated.

Lingulepis meeki, n. sp.

Shell small attenuate, marked by rather strong concentric lines and striæ of growth, and interrupted irregular radiating striæ.

Ventral valve narrow, elongate; beak acuminate, rostral slopes long, nearly straight, passing gradually into the curvature of the anterolateral margins, and posteriorly meeting at a very acute angle; front strongly rounded. Length of valve 8^{mm}, width $3{\cdot}5^{mm}$, the widest portion being near the anterior extremity. Beak slightly upcurved, the longitudinal median line straight or even slightly concave from the apex of the beak to the middle, where it begins to slope gently to the frontal margin; transverse curvature very slight anteriorly, more convex near the beak.

Dorsal valve more convex than the ventral, linguliform; beak depressed, bluntly rounded, curving evenly and gradually to the semitruncate anterior margin.

The interior markings of this shell have not been ascertained, but the external characters are such as to make a reference to the genus *Lingulepis* more than probably correct. The flat, acute-acuminate ventral valve with its elevated or retrorse beak, which is not covered by the smaller dorsal shell, is peculiarly characteristic of *Lingulepis*.

There is a form from Texas probably identical with *Lingulella perattenuata* that might be mistaken for this species, but it is an undoubted *Lingulella* and does not show the external characteristics of *Lingulepis*. A comparison of *L. meeki* with the young and narrow specimens of *L. acuminata* shows it to be clearly distinct from that species; the posterior rostral slopes of *L. acuminata* possess a peculiar incurving which is not shown in *L. meeki*.

Formation and locality.—Middle Cambrian, upper beds of Flathead Terrane, Crowfoot Section, Gallatin Range, Yellowstone National Park.

Art. XLI.—*Seiches on the Bay of Fundy;** by A. Wilmer Duff.

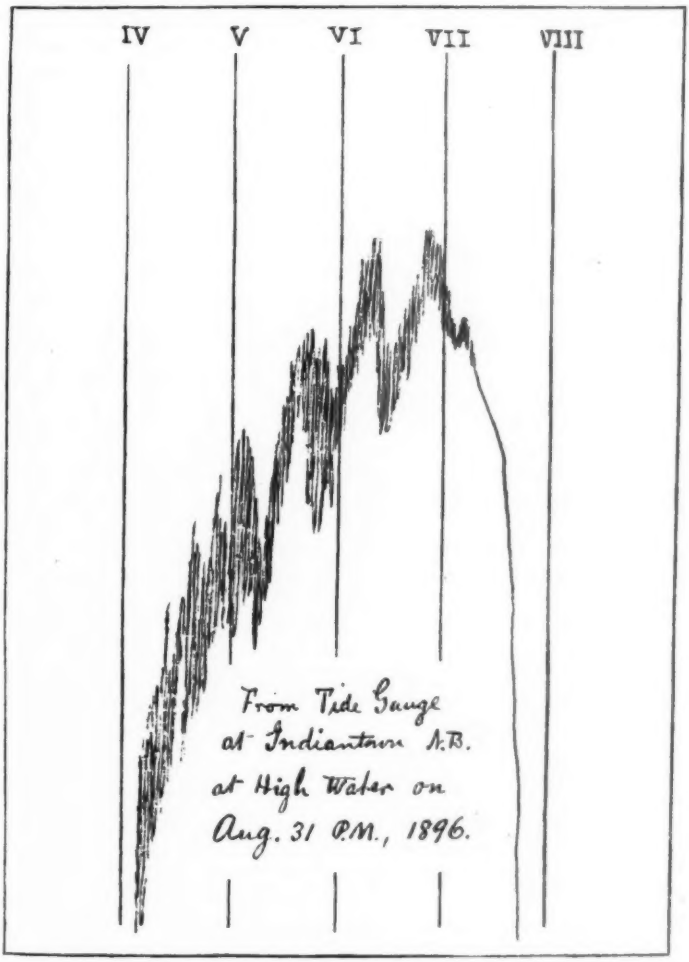
Reference has been made by several writers on tides to so-called "secondary undulations" recorded by self-recording tide-gauges. The most recent mention of the matter is that of Mr. W. Bell Dawson, engineer in charge of the Dominion of Canada Tidal Survey. He notes† these "secondary undulations" on the records of the Kelvin tide-gauge in the harbor of St. John, New Brunswick, on the Bay of Fundy. "They stand," says Mr. Dawson, "in much the same relation to the main tidal wave as a high octave would to a low musical note, when their undulations are recorded graphically. They have an amplitude which is sometimes over a foot, and a period of about forty minutes. It does not seem that any satisfactory explanation has yet been given to account for them."

The same matter came to my attention in the summer of 1896 in such a way as to suggest an explanation. While engaged with a simple form of self-recording tide-gauge in making some tidal observations on the St. John River, I obtained at a point near the mouth of the river the curious trace of high water shown in the adjoining figure. The station at which it was obtained was Indiantown, a part of the city of St. John, immediately above the very narrow outlet (only one hundred yards wide), through which the large river rushes into St. John Harbor. The day on which it was obtained (Aug. 31), was a very calm one. While watching the instrument before high tide I noticed curious little fluctuations of level, having a fairly constant period of thirty-five seconds, as determined by a stop-watch. This period I knew would be sufficient to enable the fluctuations to give a record on the drum of the instrument, whereas mere steamer waves succeeding each other at irregular intervals of a only a few seconds could not, owing to the smallness of the opening through which the water obtained access to the float, give any such trace. These 35-second undulations show themselves in the fine tracings on the left of the record. But what was quite unexpected was the series of larger undulations shown in the record and having a period of between thirty and forty minutes. It will be noticed that both series cease at nearly the same time, about half an hour after high water at Indiantown, that is about two and a half hours after high water in St. John Harbor (for the

* Abstract of a paper read before the Natural History Society of New Brunswick.

† Trans. Roy. Soc. Can., vol. i, 1895–96.

narrow gate between the harbor and the river produces a delay of two hours in high water and low water at Indiantown, which is less than a mile from the harbor). This simultaneous ceasing of the two series of undulations seems to suggest a connection between them.



As regards the slower undulations, it seemed very probable, from the similarity of the period, that they were merely the secondary undulations in the harbor, noted by Mr. Dawson, propagated into the river. These secondary undulations have usu-

ally been regarded as having some connection with the tides proper. As regards the 35-seconds undulations such a connection was clearly impossible, when regard was had to their very brief period. They evidently could not be *forced* vibrations, and if not they must be the *free* vibrations of some semi-confined body of water. Now this body could only be the small bay into which the river expands at Indiantown, nearly closed above by a part called the Narrows, and below by the narrow outlet referred to. This suggested at once that the slower undulations in the harbor had a similar origin, namely, that they were the free vibrations in another semi-confined basin. This basin could not be the harbor itself. Its dimensions are about those of the small bay at Indiantown, and its time of free swing could not be sixty times as great. The only other basin available for an explanation was the Bay of Fundy itself, limited on one side by the New Brunswick coast and on the other by the Nova Scotia coast. Hence it seemed probable that the "secondary undulations" in the harbor were the free vibrations of the Bay of Fundy along a line from St. John to the nearest part of the Nova Scotia coast. The rate of such vibrations depends on the dimensions of the basin, and this provides a means of testing the above theory.

Before applying this test, attention may be called to some similar phenomena observed long since but only recently come to my knowledge. For a great many years certain fluctuations of water level at points on Lake Geneva were known and popularly called *seiches*. After many scattered observations by Bertrand, de Saussure, Vaucher and others, a thorough study of the subject was undertaken by Dr. F. A. Forel, whose two articles* give a complete résumé of the subject. Vaucher had found that the *seiches* were most common in changeable weather, with a low barometer, and he considered them to be due to merely local temperature and consequent barometric changes, caused by rifts in clouds and variations of sunshine on the lakes. Forel added the idea that, after the disturbance of equilibrium produced by the above causes, the *seiches* are simply the subsequent vibrations of the whole lake about its equilibrium position. This theory he tested by making observations on several lakes with the use of self-recording gauges and proving the following laws:† (1) At any one place the *seiche* period is somewhat variable, but the mean is fairly constant; (2) While the water is falling at one side of a lake it is rising at the opposite side, and *vice versa;* (3) The period in the different lakes varies as the width, and inversely as the square root of the depth. He even predicted a depressed area in

* Annales de Chimie et Physique, ix, 1876.
† Phil. Mag., Series V, vol. ii, 1876.

Lake Wallenstadt because of the *seiche* period not agreeing with (3) and discovered it by soundings. He also found both vibrations parallel to the width and vibrations parallel to the length of certain lakes.

The laws of such vibrations as the above have been established both theoretically and experimentally. If t be the time of vibration, parallel to a side of length l, of the water contained in a shallow rectangular vessel with vertical sides and horizontal bottom, and of depth h, then

$$t = 2\left(\frac{\pi l}{mg} \coth \frac{m\pi h}{l}\right)^{\frac{1}{2}}$$

in which for fundamental or binodal vibrations $m=1$, for trinodal $m=2$, etc. When h is very small compared with l

$$t = \frac{2l}{m\sqrt{gh}} \text{ nearly.}$$

The above is considered to be a first approximation to a solution when the depth is variable, if h be understood to mean the average depth. Thus Forel's law (3) is completely in accord with theory.

Forel's proof of his theory was accepted as sufficient by Sir G. B. Airy.* The latter in reducing tidal observations from Malta found in them oscillations of a sensibly uniform period of 21 minutes and of magnitude considerably greater, at times, than the tides themselves. They were usually simple harmonic curves but sometimes *notched* at the top by smaller oscillations. Airy ascribed the Malta undulations to vibrations between a certain sand bank on the African coast and another on the Sicilian coast, and states that a rough calculation seemed to verify this. He also refers to similar records at Swansea.

No one else seems to have devoted much time or attention to studying the vibrations of large bodies of water, although a number of random observations have been made. As the subject seems worthy of attention, I give in a foot-note references to the literature of the subject.† In two respects, at least, the

* Phil. Trans Roy. Soc., 1878.

† Forel, Airy, Dawson—see references already given.

J. H. McFarland (Nature, Mar. 12, 1895), observed *seiches* on Lake Derravaragh.

Charles Rhodes (Science, May 7, 1886), observed fluctuations at Oswego.

J. LeConte (Overland Monthly, 1883), predicted *seiche* period of Lake Tahoe.

Ledyard (Science, Feb. 7, 1890), observed 10-min. pulsations on Lake Cazenovia.

T. D. Graham (Proc. A. A. A. S., 1883), found tides on Lake Michigan but apparently no *seiches*.

Smithsonian Contributions, vol. xii, mention fluctuations on Lake Superior.

M. P. Du Boys (Comptes Rendus, tome xii, No. 21, 25 Mai, 1891).

Century Dictionary (under "*Seiche*"), refers to similar phenomena on the Baltic.

question seems of importance. (1) A determination of *seiche* period will afford an estimate of mean depth and may assist in checking soundings or even discovering areas of depression. such as Forel found in Lake Wallenstadt. (2) Many, at least, of the often noted but (except for Airy's apparently half-forgotten remark) unexplained "secondary undulations" may be merely *seiches* on salt water. The Bay of Fundy offers special advantages for testing this latter point. The care with which it has been sounded and its great tidal range (at St. John 27 feet at springs), should enable us to apply the three following tests: (1) Calculation should give a vibration period agreeing with the period of the "secondary undulations." (2) This period should be greater at low water, owing to the less depth, than at high water. (3) Opposite sides should move in the same or in opposite phase, according as the vibrations have an odd or an even number of nodes.

Let us first make a calculation for the small bay at Indiantown. From the chart of the river we have, for the width at the point of observation (Marble Point), 2,030 feet. This is also about the width for some distance farther up; immediately below, there is a great increase of width, due to a small cove; but as this cove is very shallow while the main basin is unusually deep, it will have no practical effect on the period of vibration. For the mean depth we have the following soundings (in feet), along the shortest line to the opposite bank, 56, 60, 78, 94, 108, 136, 124, 108 the mean of which is 95 feet. Immediately above and below, the depth is slightly greater, in one place, 196 feet. Remembering that the formula for calculation is only approximate, it will suffice if 100 feet be taken as mean depth. Substituting these figures in the first formula, we get for binodal vibrations a period of 72 seconds and for trinodal 37·5 seconds. This seems to point to the 35-second pulsations being trinodal vibrations. (The less accurate formula would have given a period of 35·8 seconds.) As the tidal range is less than two feet this period should not appreciably vary with the tide.

In the case of the "secondary undulations" in the harbor, we have, for the width of the bay along a line from the tide-gauge station at St. John to the nearest point of the Nova Scotia coast, 39·8 miles. There will be a slight error in taking this as the width of the basin, for, while the Nova Scotia coast is nearly straight and unbroken, St. John harbor forms an indentation on the New Brunswick side. As it is uncertain how much deduction should be made on this account (perhaps about two miles), the above figure will be used as it stands. From the chart we obtain the following (low water) soundings between St. John and the opposite coast: 10, 4, 5, 9, 21, 27,

33, 36, 38, 40, 42, 43, 47, 50, 50, 45, 43, 43, 46, 46, 40, 40, 39, 35, 27 fathoms, or a mean of 34·4 fathoms. Reducing to feet and substituting in either formula we have, for binodal vibrations, a period of 87 minutes, and for trinodal, 43·5 minutes. Now the period of the "secondary undulations" is, according to Mr. Dawson, "about 40 minutes." If an allowance of two miles had been made for the depth of the indentation referred to, and the corresponding soundings omitted, the calculated period for low water would become very nearly 40 minutes. On the other hand, it will be shown below that the observed period is possibly more nearly 43 minutes. In any case the evidence that these "secondary undulations" are also trinodal vibrations seems strong if not conclusive.

Next, as to the difference of vibration period at high water and at low water, it can be calculated that for a difference of 20 feet (about the mean range at St. John), the period should be two minutes less at high water than at low water. To enable me to test this point, Mr. Dawson has kindly supplied me traces of all the records of secondary undulations at present available and sufficiently well defined to give a reliable estimate. Unfortunately these can hardly be considered sufficient to establish the point, but such as they are, they are given. For low water the periods are 41, 45, 45, 39, 40, 32, 49, 53, giving a mean of 43 minutes; for high water, 43, 35, 43, 40, 46, giving a mean of 41·4. The former mean is 1·6 minutes greater than the latter, but the data are insufficient to justify much reliance being placed on this argument. The apparent variability of period shown by the above figures may be readily accounted for. In the first place every slight barometric change or change of wind produces a corresponding change of level and this is superposed on the "secondary undulations," producing an apparent variation of period. Secondly, the irregularities of coast line may have an effect. Thirdly, and perhaps most important of all, different modes of vibration may co-exist in different proportions at different times, so that the resultant period may at some time be more nearly that of binodal vibration, at others of trinodal, etc. Thus, a higher mode of vibration co-existing with the prevailing mode might account for the "notches" observed by Airy in the secondary undulations at Malta.

As regards the relation of the phases of vibration at opposite sides of the bay, no information is at hand. St. John is the only point on the bay at which there is a recording tide-gauge. Another at Digby, on the opposite side of the bay, would probably settle the matter in a month; but the cost of erecting a gauge suitable for such work is considerable. I hope on some occasion, when the water is calm and "secondary

undulations" exist at St. John, to be able to obtain a record near Digby by use of a small portable gauge.

If the preceding explanation of "secondary undulations" prove correct, it will be a question of interest why the undulations here treated of are trinodal, while those of the lakes observed by Forel are chiefly binodal. No light seems thrown on the question by present hydrodynamical theory. The explanation may perhaps be found in the fact that a lake is a completely enclosed body of water, while a bay is open at one side or two opposite sides. If, as seems possible, the smaller undulations across the basin at Indiantown are produced in some way by the larger ones of the harbor entering at one end of the small bay, the result would more probably be a simultaneous elevation at opposite sides than an elevation at one and a depression at the other. This suggests that the undulations across the Bay of Fundy may in some similar way be produced by the uprush or downrush of the tides. They do not seem (as in the case of the Swiss *seiches*) to be connected with abnormal conditions of barometer; at least no such connection appears to exist in the cases whose periods are given above.

Addendum. In a recent article (Phil. Mag., Jan. 1897), Mr. C. Davison has pointed out that a considerable error may be made in extending the formula for a basin of uniform depth to one of irregular depth. In fact the true period of the trinodal vibrations considered in the preceding is

$$\int_0^l \frac{dx}{\sqrt{gy}}, \ (y = \text{depth}).$$

Changing this to the form

$$\frac{l}{\sqrt{g}} \times \left(\text{mean } \frac{1}{\sqrt{y}}\right),$$

I have calculated its value for trinodal vibrations across the Bay of Fundy, at St. John (allowing two miles as before) and find that it gives a period of 42·2 minutes.

Purdue University, Lafayette, Ind.

ART. XLII.—*On Rœblingite, a new Silicate from Franklin Furnace, N. J., containing Sulphur Dioxide and Lead;* by S. L. PENFIELD and H. W. FOOTE.

MR. FRANK L. NASON, who has been especially interested in the geology and mineralogy of the zinc deposits of Franklin, New Jersey, has recently brought to our attention a mineral from the Parker shaft of the New Jersey Zinc Company, which owing to its unusual chemical composition is of especial interest. The mineral occurs in dense, white, compact masses, which consist of an aggregate of minute prismatic crystals. These when examined with the microscope show parallel extinction and a weak double refraction, but they are so minute that the system of crystallization could not be determined. The specific gravity is 3·433; hardness a trifle under 3. A chemical analysis of this material by Foote gave the following results:

	I.	II.	Average.		Ratio.		
SiO_2	23·51	23·66	23·58	·393		5·61	5·0
SO_2	9·01	8·99	9·00	·141		2·01	2·
PbO	31·07	30·99	31·03	·139		1·99	2·
MnO	2·46	2·51	2·48	·035			
CaO	25·91	25·98	25·95	·463			
SrO	1·33	1·46	1·40	·014	·520	7·43	7·
K_2O	·16	·09	·13	·001			
Na_2O	·43	·36	·40	·007			
H_2O	6·36	6·35	6·35	·353		5·04	5·
			100·32				

The ratio of $SO_2 : PbO : H_2O$ is very close to 2 : 2 : 5, but that of the SiO_2 and the remaining bases to these constituents is not so simple. The nearest approach to a simple ratio is perhaps $SiO_2 : SO_2 : PbO : RO : H_2O = 5 : 2 : 2 : 7 : 5$, and this gives the rather complicated formula $H_{10}Ca_7Pb_2Si_5S_2O_{28}$. The water is driven off at a rather high temperature and is therefore to be regarded as hydroxyl. The formula given above may be regarded as a combination of five molecules of a silicate H_2CaSiO_4 and two of a basic sulphite $CaPbSO_4$ or $(CaO . PbSO_3)$. The theoretical composition for this formula is given below, together with the results of the analysis after substituting for MnO, SrO and the alkalies their equivalent of CaO and recalculating to 100 per cent.

	Found.	Theory.
SiO_2	23·8	22·1
SO_2	9·1	9·4
PbO	31·3	32·9
CaO	29·4	29·0
H_2O	6·4	6·6
	100·0	100·0

There must be some question as to the exact formula of the mineral, for the one proposed is complicated and the agreement between the results of the analysis and the theory is not so close as one would desire. It is not probable, however, that the mineral is a mixture, or, at least, that it contains much foreign material. If it were a mixture, one would expect the presence of some heavy lead mineral, considerably above 3·43 in specific gravity, with a lighter calcium silicate. In order to test this point some of the material was powdered, sifted to a uniform grain and placed in methylen iodide with a specific gravity of 3·29 when all of the powder sank. It is thus very definitely proved that wollastonite (specific gravity 2·9) or any related calcium silicate is absent.

The special points of interest connected with this mineral are that this is the first time that a sulphite has been observed in nature, and that silicates containing lead are very rare, having been observed up to the present time in only a few localities in Sweden.

The mineral fuses before the blowpipe at about 3 to a gray globule, and gives the pale blue flame of lead, which ceases after heating for some time. On charcoal with sodium carbonate in the reducing flame, lead globules and a coating of lead oxide are produced, and the residue reacts on silver for sulphur. With the fluxes, the reactions for manganese are obtained. In the closed tube, water is given off. The powdered mineral is dissolved with ease in acids, even when very dilute, and gelatinous silica is obtained upon evaporation. The odor of sulphurous anhydride may be obtained when the mineral is dissolved in a little hydrochloric acid, but this test is not very apparent.

Concerning the occurrence of the mineral, the following information has been received from Mr. Nason: The mineral occurs at the one thousand foot level, in or near the contact between granite and white limestone, where great veins and bunches of garnet rock are found. The associated minerals are garnet, titanite, zircon, phlogopite, axinite both massive and in drusy crystals, willemite in small, green, transparent crystals, datolite, barite, caswellite, calcite, arsenopyrite, sphalerite, rhodonite and rhodochrosite. The axinite, which occurs

in veins and pockets in the garnet rock, is in places porous and full of cavities and these are completely filled at times with the masses of rœblingite. The largest mass that was found weighed about five pounds and was about the size and shape of a cocoanut.

The ore body and the rocks in the mine are greatly shattered and show frequent slickensides, and undoubtedly the agencies which produced the shattering movements were the ones which also produced the conditions necessary for the formation of the great variety of minerals observed at this locality.

The method used in analyzing the mineral is as follows:

The mineral was dissolved in nitric acid, and silica removed in the ordinary way, substituting nitric for hydrochloric acid, so that lead might not remain behind. When heated in the air bath at 140°, however, some manganese nitrate was decomposed and converted into a higher oxide which was insoluble in nitric acid. This was filtered off with the silica, and after washing, was dissolved by treatment with hydrochloric acid. The nitric and hydrochloric acid filtrates were evaporated to dryness separately and a further trace of silica was thus removed. Lead was precipitated from the slightly acid solution by hydrogen sulphide and determined as sulphate, and manganese and calcium were determined by the ordinary methods. The small amount of strontium was weighed with the calcium as oxide and separated by treatment with amyl alcohol. For the determination of sulphur dioxide, separate portions were used. The mineral was treated with strong bromine water and hydrochloric acid, and after being dissolved and standing for some time, the sulphuric acid formed was precipitated as $BaSO_4$. The precipitate was impure, containing silica and probably some lead sulphate, and it was purified in the ordinary way by fusion with sodium carbonate and reprecipitation with barium chloride. Water was determined directly by means of the closed tube method,* and alkalies by a Smith fusion.

At the request of Mr. Nason the authors take pleasure in naming this mineral rœblingite in honor of the celebrated engineer, Mr. W. A. Rœbling of Trenton, N. J. They also take pleasure in expressing their thanks to Mr. F. L. Nason and Mr. John A. Manley of New Brunswick, N. J., for the pains they have taken in supplying material for this investigation.

Mineralogical and Petrographical Laboratory.
Sheffield Scientific School, New Haven, April, 1897.

* This Journal, III, xlviii, p. 31, 1894.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Rotation of Circularly Polarizing Crystals in the State of Powder.*—It has been shown by Landolt that the specific rotation of isotropic circularly polarizing crystalline substances, in a state of powder, may be determined in an ordinary polarimeter tube, by suspending the finely divided material in a liquid having the same refractive index and then measuring the angle through which the plane of polarization of the entering beam is turned. For the best result, the substance should be pulverized in an agate mortar and then sifted, so as to obtain particles of from 0·004 to 0·012^{mm} in diameter; and while the measurements are being made the polarimeter tube should be constantly rotated, so as to keep the particles uniformly suspended. The speed of rotation should preferably be between 50 and 80 revolutions per minute, since at higher velocities the particles of the solid are driven by centrifugal action to the sides of the tube, leaving the central portions more or less empty of them. The direction of rotation of the tube is immaterial. The author's experiments were made with both right and left-handed sodium chlorate, from 0·1 to 0·3 gram of the powder being placed in a test tube with alcohol, carbon disulphide being gradually added until the maximum transparency was reached; this requiring about two volumes of the disulphide to one of alcohol. The mixture was then placed in the tube of the polarimeter and the angle of rotation measured. To determine the amount of solid, the contents of the tube were placed in a platinum dish, evaporated to dryness and the residue weighed. In this way the specific rotation of the sodium chlorate in powder was found to be $[\alpha]_j = \pm 1{\cdot}41°$; while that of the crystals is given as 1·42° for 1^{mm}, practically the same value. Hence it appears that particles of this salt having a diameter of from 0·004 to 0·012^{mm} possess the same crystalline structure as that to which the circular polarization of the larger and completely formed crystals is due.—*Ber. Berl. Chem. Ges.*, xxix, 2404–2412, October, 1896. G. F. B.

2. *On the Oxidation of Nitrogen Gas.*—In his experiments upon the isolation of argon, Rayleigh made some observations upon the oxidation of nitrogen, which he has communicated to the Chemical Society. His first experiments were made to test the statement of Davy that the dissolved nitrogen in water is oxidized to nitrous or nitric acid when the liquid is submitted to electrolysis. The water was contained in two cavities bored in a block of paraffin and connected by a wick of asbestos previously ignited. By means of platinum terminals connected with a secondary battery a difference of potential of 100 volts was maintained in the cells, the whole being covered with a glass shade. But under these conditions, litmus showed no difference between

the two portions of water, even after fourteen days. If however the shade was removed the litmus responded in a day or two. Nor did the action of sodium peroxide, either with or without acid, effect the oxidation of dissolved nitrogen. His later experiments were directed to test the effect of pressure in the oxidation of nitrogen by oxygen gas under the action of the electric discharge. A suitable mixture of nitrogen and oxygen, standing in an inverted test tube over alkali, was sparked from a Ruhmkorff coil driven by five Grove cells; and it was found that under a total pressure of three atmospheres the mass absorbed was about three times that absorbed in the same time under the ordinary pressure. He therefore proceeded to repeat the operation on a larger scale, using a vessel of 250^{cc} and the alternate current discharge. To protect the glass, the top of which came nearly in contact with the electric flame, as well as to facilitate the absorption, the alkali was thrown on to the glass immediately above the flame in the form of a fountain, thus washing the entire internal surface. At atmospheric pressure, however, the absorption was but 1600^{cc} per hour; only one-half the rate already obtained. On increasing the capacity of the working vessel, the efficiency improved. With a bottle of 370^{cc} capacity, a rate of 2000^{cc} per hour was attained. With a larger flask, one of 3300^{cc}; while a globe of 4½ liters gave a rate of 6800^{cc} per hour. In this latter case, ten amperes passed through the primary and there was a potential difference of 41 volts at its terminals. To test the question of pressure, a glass globe having a capacity of about 7 liters was used, under a pressure of nearly an additional atmosphere, this pressure being obtained by gravity, the feed and return pipes for the alkaline fountain as well as the pipe for the supply of water to the gas-holder being carried to a higher level than that at which the rest of the apparatus stood. The rate of absorption (reduced to atmospheric pressure) was 6880^{cc} per hour, that conducted at the ordinary pressure being 6600^{cc}. Under half an atmosphere the absorption was 5600 and 5700^{cc} per hour. Hence it would appear that pressure is of advantage only where the space is too confined to admit of the best efficiency at a given pressure being reached. The author next sought to obtain a high rate of absorption by employing a powerful electric flame contained in a large vessel whose walls were washed internally by an alkaline fountain. The flame was obtained by means of a Swinburne transformer insulated with oil, which gave on open secondary nearly 8000 volts; but which when the discharge was passing fell to 2000. In its primary circuit a self-induction coil was placed provided with an adjustable core of iron wires. When in operation the potential difference at the primary terminals was only 30 volts, the current passing being 40 amperes. But since the reduction-factor—the cosine of the angle of lag—is about two-thirds, the watts expended are only about 800 in place of 1200, the product of 30 by 40; so that the rate of energy consumption was a little more than a horse-power. The working vessel was

globe-shaped and of 50 liters capacity. The neck, placed downwards, was closed with a rubber stopper carrying five glass tubes. Two of these were heavy and carried the electrodes. Two others supplied and drained the fountain and the fifth furnished the gas-mixture, 11 parts of oxygen to 9 of air, contained in a large gas-holder. The rate of absorption was about 21 liters per hour, thus giving about 25^{cc} per watt-hour. So that this apparatus works about three times as fast as the former one in which the vessel was smaller and the alkali stationary within it.—*J. Chem. Soc.*, lxxi, 181, February, 1897. G. F. B.

3. *On the Presence of Nitrites in the Air.*—A quantitative examination of the air for nitrites has been made by DEFREN. Re-distilled water, free from ammonia, nitrites and nitrates, was exposed to the air in well ventilated rooms in porcelain evaporating dishes 15^{cm} in diameter. Each dish contained 100^{cc} of water, the superficial area exposed being 95$^{sq\ cm}$. The water was examined at intervals for nitrites, the quantity being computed by comparison with a standard solution of potassium nitrite containing, per cubic centimeter, 0·0000001 gram of nitrogen as nitrite, the circumstances as regards burning gas jets and lamps being noted. Under varying conditions the quantity of nitrite found, in terms of cubic centimeters of standard, was after one hour, 2·5, 3·5 and 8; after two hours 3·5, 8·5 and 13·5; after 17 hours in the last instance 84·2; and after nineteen hours in the first and second instances 57·2 and 72·7 respectively. These results show that nitrites exist in the air where gas is burning, even in well ventilated rooms; and also that water absorbs these nitrites in quantities increasing with the time of exposure to the polluted air.—*Chem. News*, lxxiv, 230; *J. Chem. Soc.*, lxxii (ii), 94, February, 1897. G. F. B.

4. *On the Solubility of Lead and Bismuth in Zinc.*—In their investigations on the solubility of lead and bismuth in zinc, SPRING and ROMANOFF used a crucible having a hole bored in the side at a convenient distance from the bottom, and stopped with a plug during the preparation of the solution. The heavier metal is first poured into the crucible so that its level is just above the opening. Then the zinc is added and covered with a layer of fused salt or charcoal to protect it, the crucible being maintained at the temperature at which the solubility is to be determined, the contents being stirred every ten minutes. By means of a ladle a sample of the top layer is now taken. On knocking out the plug in the side, the top layer runs out and a sample of the bottom layer is ladled out. The two samples are analyzed, a table being given of the composition of the two layers at temperatures from 266° to 900°. Plotting the temperatures as abscissas and the percentages of the two layers as ordinates, two points are obtained for each temperature. One of these is the solubility of bismuth in zinc, the other the solubility of zinc in bismuth. By prolonging these until they meet, the temperature is found at which the two metals will mix in all proportions. The

critical temperature for bismuth and zinc is between 800° and 850° and that for lead and zinc between 900° and 950°.—*Zeitschr. anorg. Chem.*, xiii, 29–36, 1896. G. F. B.

5. *Traité Élémentaire de Méchanique Chemique fondeé sur la Thermodynamique.* P. DUHEM, Tome I, 299 pp, Paris, 1897. (Hermann.)—This is the first volume of a work which promises to be extensive, although there is no intimation here of what the author's complete plan may be. A short introduction, reviewing certain mathematical conceptions and forms of notation, which are used later, is followed by a division constituting somewhat more than half the whole volume treating of the fundamental principles of thermodynamics, and by a second on "false equilibriums" and explosions. The novel term false equilibrium is defined as a state of equilibrium which is experimentally realizable although the theory of thermodynamics hitherto developed declares it impossible.

An abstract from the author's preface will serve to give a better idea of his aims than any criticism of the more or less considerable fragment before us.

After attributing to Berthollet the enunciation of the truth that a study of the laws which govern chemical combinations and decompositions is inseparable from the study of those which govern changes of physical state, which more comprehensive study constitutes the science of chemical mechanics, he writes: "Berthollet, in accordance with the ideas prevalent at his time, demanded the principles of this science from Statics and Dynamics . . . . . Sainte-Claire-Deville recognized that the principles of chemical mechanics must be sought in Thermodynamics; developed and defined by Horstmann, by Montier, by Gibbs, by Helmholtz, by a host of other physicists, his idea has given birth to a body of doctrines ample and fruitful, the *chemical mechanics founded upon thermodynamics.* It is this body of doctrines which we propose to develop."

6. *Elements of Theoretical Physics;* by DR. C. CHRISTIANSEN, translated by W. F. Magie, Ph.D., 339 pp. New York, 1897. (The Macmillan Company.)—This is a book covering a wide field in mathematical physics and is certain to prove useful. The style is condensed but clear and direct. The translator's work is so well done—from the German of Müller—that a reader would possess unusual critical faculties who could recognize its source as other than in a writer of English.

A book of such moderate size and covering so wide a range of subjects must of necessity be very concise, far too much so to be of easy digestion by even the cleverest students. As an illustration of this condensation we may cite Chapter xii, Refraction of light in isotropic and transparent bodies. This contains an introduction followed by sections on Fresnel's formulas; the electromagnetic theory of light; equations of the electromagnetic theory of light; refraction in a plate; double refraction; discussion of the velocities of propagation; the wave surface (two sections);

the direction of the rays; uniaxial crystals; double refraction at the surface of a crystal; and double refraction in uniaxial crystals, all of which are contained in thirty-seven pages. The chapter which precedes this treats somewhat compendiously of electrical oscillations in a space of less than fifteen pages. It is clear that a student can hardly be expected to master these portions of mathematical physics with no other help, but, on the other hand, as an aid in coördinating his knowledge and as a guide in the choice of a rational and consistent system of notation the book is of very great value.

7. *Theory of Physics;* by JOSEPH S. AMES, Ph.D., pp. xvii +513. New York, 1897. (Harper & Bros.)—As stated in the author's preface, this book is designed for use as a text-book by college classes in elementary physics in connection with a course of experimental lectures and recitations. The methods of presentation are clear, the arrangement logical and, as a whole, the book seems well adapted to its purpose. The portion dealing with mechanics is especially to be commended; the definitions are consistent and philosophical, and the way in which the analogies between translation and rotation are brought out and insisted upon is admirable.

The section upon electricity and magnetism is largely written from the Faraday-Maxwell point of view, and the ether is frequently referred to as the medium of electrical actions; it seems strange, however, to find no mention made of the sole ground for this belief; viz. the entire similarity between light and electrodynamic action. So great a generalization would seem to merit some discussion even in an elementary text-book, published twenty-five years after Maxwell and nearly ten years after Hertz.

Problems in the reflection and refraction of light are mostly treated by considering wave surfaces and their curvatures instead of rays and focal lengths. This treatment is much to be preferred, not only because of its direct reference to the nature of light, but also because it leads to methods of much greater elegance and power in the solution of complicated optical problems.

The work, as a whole, is a decided improvement upon the ordinary text-books of elementary physics and will no doubt be welcomed by teachers of that subject. H. A. B.

8. *The Outlines of Physics, an Elementary Text-Book;* by E. L. NICHOLS, pp. 452, New York, 1897. (The Macmillan Company.)—This is an elementary treatise for use in preparatory schools. It aims to combine in one volume the laboratory manual and the text-book, being intended for a one year's course. The best feature of the book is that part devoted to the laboratory directions. There are above a hundred experiments, carefully worked out and explained, the apparatus used being simple and inexpensive. The emphasis laid on the tabulation and interpretation of the quantitative results, whenever obtained, is very welcome, on account of its conspicuous absence in most elementary

text-books. The theoretical portions perhaps suffer by contrast with the experimental on the score of lack of concise definitions. To take but one instance, there is apparantly no definition given of a fluid, and the distinction between fluids and liquids, though tacily assumed, is not definitely stated. It also might be wished by some that the author had introduced the conception of lines of force in the section on electrostatics and not have left it until magnetism, or rather the magnetic action of a current, was taken up. However, this and other points more or less pedagogical in nature, suggested by the arrangement adopted by the author, are after all matters of personal opinion. The book is admirably adapted to schools with limited laboratory equipment, and is probably in the first rank of its class. L. P. W.

II. Geology and Natural History.

1. *Geological Survey of Canada.*—The following parts of the Annual reports for 1894, 95 and 96 have been issued :

Maps to accompany annual report, vol. vii, 1894. 556 British Columbia—Kamloops sheet geology; 557, same, topography, etc.; 567, same, Finlay and Omenica rivers. 571, Quebec—Southwest sheet, "Eastern Townships" map (Montreal sheet.) 561, New Brunswick and Nova Scotia—Sheet 4 N. W.—(Cumberland Coalfield sheet) Surface geology. 562, same, sheet 2 S. E.—(Richibucto sheet) Surface geology. 563, New Brunswick and Prince Edward Island—Sheet 5 S. W.—(Buctouche sheet) Surface geology.

Part J. Ann. Rept., vol. viii. Report on the geology of a portion of the Laurentian area lying to the north of the island of Montreal; by Frank D. Adams.

Part S. Ann. Rept., vol. viii. Section of Mineral Statistics and Mines. Annual Report for 1895; by Elfric D. Ingall and L. L. Brophy.

Summary Report of the Geological Survey Department for the year 1896, George W. Dawson, Deputy Head and Director. In this last document is found a brief summary of the more important results of the survey during the year. The Director remarks : "During the past year very notable progress has been made in development of the mineral resources of Canada, both in the way of actual work and in attracting the attention and interest of capital. British Columbia has begun to evidence its value as a permanent producer of the precious metals, in a manner long foreseen by those who have paid attention to its geological structure and position. In Ontario, wherever the Huronian system is developed and has been examined, valuable mines—more particularly those of gold—are being discovered and opened up. In Nova Scotia, renewed interest has been shown in gold mining, etc. (p. 7.)

Corundum has been discovered in Hastings Co., Ontario. Investigations in the peninsula of Labrador have revealed Cam-

brian rocks with beds of iron ore. In this region it was found "that the rock-striations indicated a flow of ice, during the glacial period, from the vicinity of the present water-shed, both to the westward and to the eastward, nearly in conformity with the general slope of the surface."

The boring for petroleum at Athabasca landing, Alberta, was carried down to a depth of 1770 feet with great difficulty, and at last the work was stopped by the caving in of the soft shales so as to make further penetration impossible with the tools at hand. The zone of so-called "tar sands" at the bottom of the Cretaceous had not been reached. The Director remarks that the work so far accomplished "has not in the least degree tended to render the existence of petroleum even at Athabasca landing, more doubtful than before." (p. 14.)

Mr. Chalmers reports two glacial clays in the St. Lawrence valley region, and has observed "a series of three shore-lines on the south side of the St. Lawrence, facing the open St. Lawrence valley intermittently extending from the Gulf to the international boundary in the vicinity of Lake Champlain, at heights of 600 to 625 feet, 700 to 720 feet, and 875 to 885 feet (aneroid measurements). These shore lines are believed to be marine and to indicate margins of the sea in the St. Lawrence valley in the Pleistocene period. (p. 82.)

In Mr. Chambers' report we notice also the following interesting fact:—"Dislocations or slips of the slates over each other, along certain zones or bands, since the glacial period, were observed in a great number of places, the displacements ranging in extent from two or three inches to five or six feet. One of the most remarkable examples of these movements in the rocks, was seen in the southern part of the seigniory of Aubert Gallion, Beaver County, where a band of slates from three to four feet thick and several hundred yards in length had sustained an upward shore of nearly six feet above the general level of the glaciated rock-surface, as evidenced by the parallel and well-marked striæ." (p. 83.) This recalls similar phenomena recorded by G. F. Matthew in the suburbs of St. Johns, New Brunswick.

H. S. W.

2. *Boletín del Instituto Geológico de México—Nums.* 4, 5 *y* 6. *Bosquejo Geológico de Mexico;* by Director JOSÉ G. AGUILERA. pp. 1–272, plates i-v, 1897.—This volume contains detailed reports of reconnoissance surveys (itinerarios geológicos) of the country by several members of the survey staff and a second part by the director presenting the classification of the formations with lists of fossils and descriptions of the rocks comprising them. A brief obituary notice of Ingeniero Antonio del Castillo, the founder and first director of the Instituto Geológico de México, is inserted at the beginning of the volume. H. S. W.

3. *An introduction to Geology;* by WILLIAM B. SCOTT, pp. 1–563, figs. 1–169, plates i–xii. New York, 1897. (The Macmillan Co.)—Professor Scott has supplied a much felt need, in giving

us an admirable class book in geology, which has enough of detail and not too much for the ordinary college student. In general the subject has been developed with much skill and a good sense of proportion, though in abbreviating the treatment too much has been omitted in some cases. For instance, geysers are dismissed with only ten lines, and discussed quite separate from the chapter on volcanoes, under the general subject of "running waters."

The subjects are treated in chapters, each one of which can be studied by itself, and, in general, sufficient definiteness of statement is given to enable the student to form correct notions of the topics discussed; but to the writer the arrangement seems too discontinuous for the best interests of a class, unless it be used to supplement a course of lectures in which the personality of the instructor appears. A want of continuity of treatment is felt, for instance, in the discussion of glaciers; thus on pp. 104–116, as one of the destructive processes; ice deposits are considered again, pp. 153–159, as reconstructive processes, and ice erosion is discussed again on pages 310–312. Again, the separation of the chapters on land sculpture, adjustment of rivers and cycles of erosion, quite distant from the destructive and reconstructive processes of dynamical geology, makes a break in continuity, which is useful in an exhaustive treatise on geology, but appears somewhat artificial in an introduction to the subject.

The illustrations of the book are chiefly new, fresh and from American sources, in this particular making the book a decided addition to text-book literature. A number of excellent photographs of typical regions and structures are reproduced, many of them admirably, but this method of reproduction has its limitations, such pictures as those of Mauna Loa and Mt. Shasta and some others (as figures 34, 120, 128) are too indistinct to have much scientific value. H. S. W.

4. *Glaciers of North America.* A Reading Lesson for Students of Geography and Geology; by ISRAEL C. RUSSELL, pp. 210, Boston and London, 1897. (Ginn & Co.)—It is not many years now since very little was known of glaciers on this continent, except those of the Greenland coast in the far north. Recent explorations, however, have shown that there are true glaciers even in the Sierras of California, while those of Alaska have been extensively studied. In this work, particularly in the latter field, the author has done much himself, and the present volume is a very interesting summary of the entire subject, which will appeal to the popular reader as well as to those interested in the strictly scientific side. Of the concluding chapters, one discusses the various hypotheses advanced to account for the movement of glaciers, and another treats of the subject of the life-history of a glacier.

The book is admirably illustrated and the half-tone reproductions from photographs add much to its interest and value.

5. *A Treatise on Rocks, Rock-Weathering and Soils;* by GEORGE P. MERRILL, pp. 411. New York and London, 1897. (The Mac-

millan Co.)—The author rightly remarks that he does not need to apologize for the presentation of this volume, since it deals with a subject which has never been systematically presented in a single volume before. The value of the work is largely increased by the fact that it includes not simply the results that have been given to science by others, but those which the author himself has obtained through a number of years of investigation. The subject is one of the highest interest, not only from the strictly scientific side, but also to those who are concerned with the use of building stones in general.

Part I discusses the mineral constituents of rocks, also their general characters and occurrences. Part II gives a summary and classification of the different kinds of rocks. Part III, which contains the most original portion of the work, is devoted to the weathering of rocks, with a discussion of the various methods by which this is accomplished, and the results in various cases as applied to rocks of different natures. The manifestations of weathering, the rate at which it takes place, and other points are also treated of. Part IV treats of the transportation and redistribution of rock debris. Part V is devoted to the "regolith" or mantle of unconsolidated material which covers a large part of the earth's surface, including both sedentary and transported materials. A particularly interesting portion of this chapter is that given to a discussion of soils in their different aspects.

This brief summary of the contents of this admirable volume will give a partial idea of what it contains, but a thorough appreciation of its value and originality can only be gained by careful perusal. Numerous excellent illustrations accompany the text.

6. *Elemente der Mineralogie begründet von Carl Friedrich Naumann.* Dreizehnte, vollständig umgearbeitete Auflage von Dr. FERDINAND ZIRKEL. I. Hälfte: Allgemeiner Theil, pp. 386. Leipzig, 1897. (Wilhelm Engelmann.)—Twelve years have now passed since the twelfth edition of Naumann's *Mineralogie* was issued by Prof. Zirkel. During this time great progress has been made in the science, as well on the theoretical as on the descriptive side. Mineralogists will, therefore, welcome warmly this admirable treatise in its new and thoroughly revised form. The first half, discussing the crystallographic, physical, and chemical properties of minerals, is now given to the public, and the second half, containing the description of species, is promised by the end of the present year.

7. *An Introduction to the Study of Meteorites*, with a List of the Meteorites represented in the Collection. British Museum (Natural History), Mineral Department, 1896.—Dr. Fletcher has recently given to the public a revised edition of the catalogue of the British Museum collection of meteorites, including a considerable number of specimens not noted in the catalogue of 1894.

8. *Pseudomorphs after halite from Jamaica, W. I.;* by E. O HOVEY (communicated).—In some material collected in Jamaica, W. I., and presented to the American Museum of Natural History by Francis C. Nicholas are some pseudomorphs after halite which are worthy of note. The outward form is the ordinary cube of the salt crystal with cavernous or hopper-shaped faces, and groups made up of from two to five individuals occur in the series. In most cases the replacing material is calcite with a considerable admixture of clay, in some gypsum (selenite) forms the pseudomorph and in others there is a shell of clayey calcite from less than $0{\cdot}5^{mm}$ to several millimeters in thickness, preserving the outward form of the salt crystal and surrounding pure selenite or a mixture of selenite, clayey calcite and iron oxide. The specimens are from a bed of hard, gray clay or shale from a short distance west of Easington, Parish of St. Thomas (formerly St. David) and are of further interest on account of the statement made by J. G. Sawkins in his Reports on the Geology of Jamaica, p. 37, that "deposits of mineral salt are not found in the island." Sawkins (p. 54) mentions gypsum, however, as occurring near Easington in beds between the Carbonaceous shales and the yellow limestone.

9. *Flora of the Southern United States: containing an abridged description of the Flowering Plants and Ferns of Tennessee, North and South Carolina, Georgia, Alabama, Mississippi, and Florida: arranged according to the Natural System;* by A. W. CHAPMAN, M.D., LL.D. Third edition. Cambridge, Mass. Cambridge Botanical Supply Company. 1897.—The title above given indicates sufficiently the nature of this work. The revision incorporates considerable new material, and, still, by judicious abbreviation and condensation, keeps within convenient form. To the increasing numbers of visitors to health resorts in the southern states, and who have anxiously but vainly sought for copies of the two previous editions, the present revision will be very acceptable. There is little doubt that a great deal remains to be done in the elucidation of our southern flora: Dr. Chapman's useful treatise will long serve as a basis for such local research and a most handy work of reference. We trust that the venerable botanist may long be spared to carry out his intention of publishing further issues as occasion may require. G. L. G.

10. *Neural Terms, International and National;* by BURT G. WILDER. (From the Journ. Comp. Neurology, vol. vi, pp. 216–352, 1897.)—This paper, the latest of Professor Wilder's contributions to a revision of anatomical nomenclature, is largely devoted to an unfortunate controversy with Professor Wilhelm His and the "Anatomische Gesellschaft." The principles of Professor Wilder's nomenclature are very fully discussed and elucidated and a useful list of anatomical terms applied to the nervous system is given.

III. Miscellaneous Scientific Intelligence.

1. *National Academy of Sciences.*—The following is a list of the papers entered to be read at the meeting of the Academy held at Washington, April 20–22:

Adelaide Ward Peckham, M.D.: The influence of environment upon the biological processes of the various members of the colon-group of bacilli.—An experimental study.

T. C. Mendenhall: On the energy involved in recent earthquakes.

T. C. Mendenhall and A. S. Kimball: On a ring pendulum for absolute determinations of gravity.

S. C. Chandler: On the variation of latitude.

Theodore Gill: The position of the Tarsiids and relationship to the phylogeny of Man.

A. A. Michelson and S. W. Stratton: A new harmonic analyser.

J. K. Rees, H. Jacoby, and H. S. Davis: Variation of latitude and constant of aberration from observations at Columbia University.

A. Agassiz: On recent borings in coral reefs.

Arthur W. Wright: Notes of experiments upon the Röntgen rays.

S. P. Langley: Biographical memoir of G. Brown Goode.

H. L. Abbot: Biographical memoir of Thomas L. Casey.

H. P. Bowditch: Biographical memoir of Charles E. Brown-Séquard.

J. W. Gibbs: Biographical memoir of Hubert A. Newton.

G. K. Gilbert: Biographical memoir of George H. Cook.

2. *Microscopic Researches on the Formative Property of Glycogen. Part I, Physiological;* by Charles Creighton, M.D. Formerly Demonstrator of Anatomy at Cambridge; pp. 152, with five colored plates. London, 1896 (Adam and Charles Black).—Dr. Creighton's work emphasizes the prominence of glycogen in young embryonic tissues, especially at the centers or points of rapid growth and at a time in fœtal life when the vascularity of the part is hardly established. From the observations made, the author inclines to the view that the glycogen of embryonic tissues possesses a kind of intangible power which makes it the forerunner and pioneer of new growths without, however, losing its own molecular identity and without becoming an integral part of the tissue; a view which physiologists will have some difficulty in accepting in its entirety. R. H. C.

3. *The Tutorial Statics;* by Wm. Briggs and G. H. Bryan; pp. 260. London, 1897 (The University Correspondence College Press).—One of the University Tutorial Series, whose object is to provide candidates for the London University Examinations with so much as is necessary to prepare for those tests without offering any opportunity for acquiring superfluous knowledge. Aside from its place in the series there seems no particular reason why it should have been written. It is as good as a number of other recent English books on the same subject, and no better.

W. B.

4. *Royal Society of London.*—Professor J. Willard Gibbs of New Haven has recently been elected a fellow of the Royal Society.

OBITUARY.

EDWARD DRINKER COPE, Professor of Zoology and Comparative Anatomy in the University of Pennsylvania, died at Philadelphia on the 12th of April in the forty-sixth year of his age. Professor Cope had devoted his life to research in the fields of zoology and paleontology, chiefly, of vertebrates.

Beginning his career as a medical student in the University of Pennsylvania, he continued his studies in comparative anatomy in the Philadelphia Academy of Sciences, the Smithsonian Institution and in Europe. In 1866 he became a professor in Haverford College. He was on the Geological Survey of Ohio in 1868. For many years after this he was engaged in explorations in the west; in connection with the Hayden Geological Survey, 1872, etc., with the Wheeler Geographical Survey, 1874, and thereafter either at his own expense or under the auspices of some one of the surveys. He collected a vast number of vertebrate fossils, the descriptions and illustrations of which filled a series of large volumes.

His earliest paleontological studies were on the fossil vertebrates of the Cretaceous greensands of New Jersey. These investigations were followed by others on the Tertiary vertebrates of Maryland and North Carolina. The remains described from these fields were chiefly reptiles and marine mammals. In Ohio his discoveries were among the Carboniferous vertebrates. In Kansas he made large additions to knowledge of Cretaceous fishes and reptiles. The Eocene formations in the Bad Lands about Fort Bridger and the Green River basin yielded him many new species. From the White River beds in Colorado he obtained some seventy or more new mammals. His explorations in New Mexico, Nebraska, Montana and Oregon still increased the species, genera and families of known Mesozoic and Tertiary vertebrates; and from Texas were taken many new Permian and Carboniferous forms.

Although his interest extended over all departments of vertebrate structure, his greatest additions to knowledge were made to the Reptiles, Batrachia and Mammals. He did not confine his attention alone to fossil vertebrates. He prepared a systematic revision of the class Batrachia and was completing a similar revision of the class Reptilia at the time of his death.

For a number of years Professor Cope has been the chief editor of the "American Naturalist," a periodical devoted to general Natural History, and he contributed many papers discussing the philosophical problems of biology. He is, perhaps, most prominently distinguished among philosophical biologists by his ardent advocacy of the theory of consciousness as the prime factor in evolution.

His published works small and large are said to exceed 350 in number. He was a member of many scientific societies in this country and abroad; he was elected a member of the National Academy of Sciences in 1872, and at the time of his death was President of the American Association for the Advancement of Science.

MATTHEW CAREY LEA died at his residence on Sunset Avenue, Chestnut Hill, Philadelphia, on the 15th of March, 1897. He was born in 1823, and was the eldest son of Isaac Lea, the publisher, well known as a geologist and mineralogist, but especially as a conchologist, in connection with his investigations on the genus Unio. Mr. Lea was a member of one of the old Quaker families of Philadelphia, his ancestor John Lea who was an active member of the Society of Friends having come to this country with William Penn in 1700. Carey Lea was educated at home by private tutors and studied law in the office of the late Wm. M. Meredith, being subsequently admitted to the bar. On account of chronic ill health, however, he never practised his profession. His early associations giving him a special interest in scientific matters, he entered the laboratory of Professor James C. Booth and there acquired great proficiency in chemistry. To this science he devoted his life, his chemical researches being numerous and important, by far the greater number having been published in the pages of this Journal. He was elected to membership in the National Academy of Sciences in 1892 and the list of his more important papers then published contained fifty four titles. These investigations for the most part related to the chemistry of photography, and especially to the action of light and other forms of energy upon silver salts. He described photo-bromide and photo-iodide of silver and in 1887 published a paper on the "Identity of the photo-salts of silver with the material of the latent Photographic Image." His most remarkable discovery however, made in 1889, was that silver is capable of existing in three allotropic states. The first is allotropic silver proper, "which is protean in its nature, may be soluble or insoluble in water, may be yellow, red, blue or green or may have almost any color but in all its insoluble varieties always exhibits plasticity; that is if brushed in a pasty state upon a smooth surface its particles dry in optical contact and with brilliant metallic luster. It is chemically active." The second is intermediate in character, may be yellow or green, always shows metallic luster, is never plastic, and is chemically indifferent. The third is ordinary silver. In 1868 Carey Lea published a "Manual of Photography" which reached a second edition in 1871.

Henry Charles Lea the eminent historical writer, was a brother of Carey Lea. His first wife was a Miss Bakewell of Cincinnati who died in 1881 leaving a son, George H. Lea, who still survives. His second wife was Eva Lovering, a daughter of the late Professor Lovering of Harvard. G. F. B.

JOSEPH F. JAMES died on the 29th day of March at Hingham, Mass. Dr. James published several papers on botanical, paleontological and geological subjects, was a fellow of the American Association for the Advancement of Science and an original fellow of the Geological Society of America.

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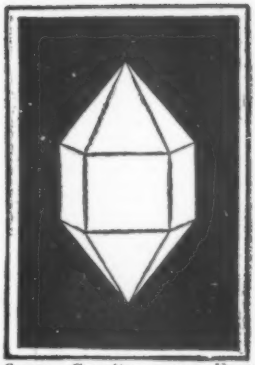

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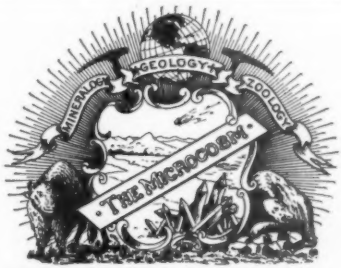

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(1) Described in this Journal Sept., 1895. (2) Ibid Nov., 1887. (3) Proc. Roch. Acad. Sci., Vol. 1. (4) This Journal Aug., 1888. (5) Ibid July, 1893. (6) Ibid June, 1894.

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